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Analysis of Yale Cold-Fusion Experiment

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Introduction

The following represents an analysis of some of the data from a cold-fusion experiment done at Yale in the fall of 1989. The purpose of the experiment was to see if proton-recoil spectra from liquid scintillators could provide adequate information about either neutron singles rates, or the time-structure and size of the neutron bursts which have been observed in other experiments.

The Experiment

The data consist of pulse heights and pulse shapes (widths) for two groups (Up and Down) of neutron detectors, each consisting of one central counter of 10% efficiency, and five ring counters of ~0.8% efficiency each. Relative times between each ring counter and its corresponding central counter were recorded. A latch bit for each detector indicated whether that detector had fired. The trigger requirement was apparently a hit in either center detector.

This analysis has been made difficult by the lack of any logic or electronics diagram for the experiment, which necessitated much detective work and frequent digressions.

Neutron Singles

With moderately -constricting two-dimensional cuts on histograms of pulse height versus pulse shape, unvetoed neutron-singles rates were on the order of 100-200/hr for each detector, for both foreground and background runs. Putting tight time-of-flight cuts on center-ring coincidences provided a measure of neutron efficiency and yielded 90% for these cuts. The quality of pulse-shape discrimination between neutrons and gammas was very good, especially at low pulse heights, and in both data and background runs, very few of the surviving events appeared to be gammas.

Although the veto counters were advertized as only 65% efficient, only 10% of the singles events were vetoed, indicating that making the veto more efficient will probably not gain much.

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Examination of gated pulse-shape/pulse-height spectra for background runs (Fig 1.) shows that most of accepted events were probably real neutrons. Looking just at the center detectors yields the following neutron singles rates:

Table 1:

run	type	100×Neut/Good
42	FG	4.92 ±0.12
43	FG	5.02 ±0.12
44	FG	4.78 ±0.12
45	FG	4.83 ±0.12
46	FG	4.95 ±0.12
47	FG	4.60 ±0.12
53	FG	4.75 ±0.12
total	FG	4.86±0.04
40	BG	4.88 ±0.12
41	BG	5.08 ±0.15
53	BG	4.53 ±0.16
total	BG	4.84 ±0.10

Note the variation of as much as 0.5% from one background run to another is adequate to mask a real signal of 10 neutrons/hr. This points out a real weakness in the experiment. Previous experiments have been done with adjacent simultaneous background measurements.

Because of these high singles rates, and the variability of background, it is necessary to see if coincidences can reduce the background.

Note that for real neutrons of proper energy, the probability of coincidences should be exactly the same for background as for foreground events. The only purpose coincidences can serve in detecting neutron singles is to put tighter constraints on the energy and location of origin of the neutrons.

Figure 2 shows particle identification (PID) vs time of flight for two ring detectors (U3 and D5). The PID value is based on the result of neutron gate tests NG and the latches CR, which assign the following values:

value	NG	CR
0	Bad pulse	no latches
1	Neutron	ring latch only
2	Gamma	center latch only
3	OK	both latches

The PID value in the graph is then PID = 8CR + 4NG(ring) + NG(center). Good events are concentrated in the region labelled 'gamma C,R', while deadtime is measured by the region 'bad R'. The scale for detector D5 (#10) TOF is $10\mu s$., while that for U3 is $\sim \pm 60ns$. See below on TDC problems for further discussion of Figure 2.

Appendix A is similar to Table 1, but for ring counters, where added coincidence constraints could further eliminate non-neutrons. The ratio of background neutrons to gammas was roughly 1:100, although there was substantial variation from detector to detector, and D1 could not be measured reliably at all this way due to a large number of obviously spurious events in the denominator.

Requiring center-ring coincidences with loose time-of-flight cuts yielded a rough neutron coincidence rate of 2/hr for both background and foreground, with an inferred neutron signal of -0.6 ± 0.4 The same comments apply as to Table 1, with the added qualification of very low statistics as well.

Although one may draw some conclusion from the agreement between foreground and background for averages of many runs, one should keep in mind that runs showing measureable singles rates have generally been rare and ephemeral, and could tend to be lost when averaging over many runs or for a long duration.

Multiplicities

Previous experiments suggest that the signal consists of a small number of bursts of many neutrons, each burst with a time scale of less than $50\mu s$. This allows a measurement technique with different systematics than neutron singles, and which offers another opportunity to extract information from the data.

A burst *might* generate immediately adjacent events, but I assume that the acquisition system was incabable of accepting back-to-back events, due to acquisition deadtime. Unless the electronics is specifically designed to handle multiple events,* the digitizing time for ADC's and TDC's will preclude acquisition of subsequent events for 50-100 μs. If this was the case, then the only way to recognize bursts would be to look for multiple neutrons within an event. Since the event gate width was $10\mu s$, the system was probably incapable of detecting an entire burst of longer duration. *as at LAMPF; see Caffrey report

Since one of the purposes of the experiment was to look for neutron multiplicities, it is important that the response to multiple events be well understood. I have written a MonteCarlo program to compute the probability distributions for various neutron burst multiplicities. The results have been checked for consistency and against predictions where possible. The calculations assume a central counter of 10% efficiency and 10 ring counters of 0.8% efficiency as described in the Yale 'Interim Report'. I conclude that the efficiencies predicted in the report are grossly overoptimistic. The proper expression, verified by MonteCarlo, for the probability $P_{M,D,\epsilon}(K)$ of having K detectors hit by exactly 1 neutron, and no other hits, given a burst of M neutrons and D detectors of efficiency ϵ, is

$$P_{M,D,\epsilon}(K) = \binom{M}{K}\binom{D}{K} K!\epsilon^K (1 - D\epsilon)^{M-K} \tag{1}$$

where $\binom{M}{K}$ is the usual binomial coefficient. Other hit criteria or differing efficiencies can make the expression much more complicated. The report uses an expression which can become much greater than one, and is not even a valid approximation to the above expression. Note that approximations for small ϵ will involve terms with $M\epsilon$, which can not be assumed to be small.

In addition, the expression used in the report cannot be uniquely applied to a system with detectors of differing efficiency. For example the efficiency for 11 detectors could be for 10 ring and 1 central or 9 ring and 2 central detectors. For these reasons, the question is most reliably handled by a MonteCarlo program. For large burst multiplicities, the probability that both central counters are hit becomes large, and it is advisable to limit the definition of detector multiplicity to that for the ring counters.

As an example of the magnitude of the above error, the report assumes the probability for detecting 5 neutrons (assume 2 central and 3 ring) out of a burst of 20 to be 100%. In contrast, the actual probability for <u>not</u> detecting *any* neutrons in ring counters would be

$$P_{20,10,0.008}(0) = (1-0.08)^{20} = 18.9\%$$

which is an obvious contradiction. The MonteCarlo predicts a value of 18.4% for $P(0)$ and a probability of only 13% for getting hits in 3 or more ring detectors with such a burst.

The calculated probabilities of various ring-detector multiplicities for bursts of 20, 50 and 125 neutrons are listed in Appendix B. The tables consist of probabilities for detecting from 0 to 10 ring-detector hits assuming various imposed conditions on multiple hits in both the central and ring detectors. 10000 events, or bursts, of specified multiplicity were generated and the number of single and multiple hits for each counter accumulated. Tables for both 0.8% and 0.5% efficiencies (labelled 'ei'), and deadtimes ('ddtm') of 0,0.1, and 1, were computed to estimate the effect of possible electronic and gate inefficiencies (see 'Inefficiencies' below).

The conditions for the central counter are: 'not required', 'any' hit, and 'sngl' hit only. The last is for cases requiring pulse-shape and TOF discrimination. The ring counter criteria are 'any', 'sngl', in which detectors with more than one hit are excluded, and 'only', in which an entire event is excluded if any ring detector has more than one hit.

If a detector has already been hit, subsequent hits have a probability of 'ddtm' of being accepted. The ring-hit probabilities are normalized to the total number of bursts. In addition to probabilities for N hits, the accumulated probabilities for N hits or greater are also shown. The expectation value for number of hits is shown at the bottom.

Equation (1) is evaluated under the column labelled 'predict'. The criteria that the equation assumes are: the center counters 'not required', and ring counters are 'only'. Equation (1) also assumes that 'ddtm' = 1.0.

Each neutron was allowed to hit *only one* each of ring and central detectors. (Thus the actual system may have a greater probability for multiple events). The program assumes only a single center counter, but because of the relatively large efficiency, the extension to two center counters should not change the results significantly. (Compare the 'not required' and the 'any' probabilities.)

The deadtime of the pulse-shape discriminators was advertized as 90ns, which is extremely good considering their good resolution. With an event gate of 10μs., this would imply an inefficiency of $\approx 1\%$. If bursts are narrower than the event gate, the inefficiency could increase to a maximum of 1.0 for burst durations of 90ns. or less.

Note that for large multiplicities and high dead-times there are non-negligible probabilities for detecting more than one neutron in a detector. In fact, for ddtm=1, the probability of two hits in one detector is exactly the probability of one hit each in two detectors.

Multiplicity Data

Requiring latches for center-ring coincidences yielded several hundred events per hour with ring multiplicities from 3 to as high as 9. Applying pulse-height/pulse-shape cuts eliminated *all* events with multiplicity 3 or higher from both foreground and background. I conclude that the high multiplicity events mentioned in the report are due to the looser cuts allowing some nominal gammas through.

The following table summarizes the effects of various applied cuts on the hourly data rate in ring detectors.

Table 2:

CRNTM	Fg(/hr)	Bg(/hr)
	~4000	~4000
XX	~100-200	~100-200
XX	~200	~200
XXX	~10	~10
XXXX	~2	~2
XX 2	0.5±0.10 (28 evts)	0.28±0.09 (7 evts)
XX 3	0	0
XXX 2	0.25±0.08 (14 evts)	0.1±0.05 (3 evts)
XXX 3	0	0

where
C is Center-detector latch
R is Ring-detector latch
N is Neutron pulse-shape/pulse height cut
T is loose Time-of-flight cut
M is Multiplicity for specified cut

Figure 3. shows the distribution of times of flight for all good-latch combinations for all events with neutron multiplicities of one or greater, for foreground (14 events) and background (3 events). There are more than 17 (times multiplicities) points because some good latch events did not satisfy neutron cuts but were still included in the time distribution. The time binning is 256 channels, and the background width is not significantly different from that of the foreground.

It is important to note that time-of-flight cuts cannot be applied to multiple events because only one start time from each center counter would be valid. (Of the 17 events that satisfied the CRN2 cut, only 2 occurred in different groups.) This is a serious problem, because it means that at medium burst multiplicities there is a good chance that the second center counter will fire more than 500ns. after the first, its latch will not be set, and time information will not be available for any ring counters in that group. This is an example of the difficulties encountered in trying to analyze data without logic or electronics diagrams.

Pulse information from the center counters is also not useful at high multiplicities because it is liable to be piled up.

A ring multiplicity of 3 corresponds to a multiplicity of 5 in the 'Interim Report'. The report finds one event of this multiplicity in run 45. My cuts give this event a multiplicity of 2 because the particle in U5 falls just outside the 'neutron' gate (ph = 720, ps = 1300). Figure 4.

Inefficiencies

The efficiency of the apparatus consists not only of the probability that a neutron will generate appropriate pulses, but that such pulses somehow get recorded as part of an event, and that the event survives any cuts applied. Generally the integrity of the electronics should be ensured prior to data taking, but since there is ample evidence that the experiment was assembled and conducted in haste, there is some grounds for concern about whether the electronics actually behaved as advertized. The following observations are pertinent.

- The lack of an electronics diagram means that one can not be sure what correlations are to be expected and are proper in the data and waste much time inferring them. Some problems cannot be solved and may not even be recognized without such information.
- Although the logbook mentions that the detector gains were adjusted, the pulse-height spectra show great differences between detectors. Amplifier saturation was a problem. Thresholds, which are of fundamental importance in any neutron measurement, also appear to vary among the detectors. Figure 5 shows raw spectra from Cf runs which illustrate these problems.
- Detector pedestals (Fig. 6) were not adjusted, and several were at or near zero, making any energy calibration suspect. This also makes it difficult to determine the electronic inefficiency of the detector, which prefers an unambiguous electronic zero.
- A small fraction of events had no latches (Fig. 7), which although probably not significant, is disquieting because such events should be logically impossible, and when the signal to noise is as low as in this experiment, any correlation between such losses and real data could be fatal.
- The TDC's (actually TAC's) and at least some of the ADC's were not cleared after each event. Thus a detector without a latch set would contain data from a previous event. This could be a problem if there are any spurious latches. Figure 2 shows these 'leftover' events, which can be identified by comparing unlatched TDC values with the most recent latched value.
- There was substantial crosstalk between TDC's, which was not entirely eliminated by requiring latches. Figures 2 and 8 also illustrates this.
- The TDC's were not matched. This was actually helpful in identifying crosstalk, but demonstrates lack of care in setting up.
- There was an enormous (about 30 times the nominal count rates) noise source in detector D1, which exactly simulated a good time of flight. Perhaps it was a pulser either purposely or accidentally left in. In any case, it was only identifiable by lack of ADC pulses.
- The design did not include any mechanism for determining the relative times of events in the two groups of detectors. Useful multiplicity data require latches for both central detectors if their corresponding ring counters are included. This puts a 500ns window on up-down events and probably explains why so few up-down events were observed when center latches were required. A complete understanding of the effect of the latches on multiplicity requires more MonteCarlo work.

Problems of this type may be expected from a new setup, but for a previously existing apparatus put together by a supposedly experienced group, they do not increase confidence that any results will be trustworthy.

An obvious source of electronic inefficiency was that a substantial fraction of events which had good times of flight failed the neutron-gamma discrimination cut because they had no pulse shape and/or no pulse height. Interestingly enough, for runs after run 30, when adjustment of thresholds, etc. apparently ceased, the inferred inefficiency for data and background runs held steady at about 50% (after neglecting the noise in D1), while the Cf run (34) showed an efficiency of 80%.

Since Cf was used to determine efficiencies, the correction to the efficiency due to deadtime depends on what the correction was for the specific Cf runs used. Without more information on what actually went into the efficiency caclulations I cannot choose a deadtime correction. Based on the observed variation both in the signals and the neutron detection rates, I am inclined to be suspicious that a single value of 0.8% is claimed to apply to all. Detector U3, for instance seemed to have less than half the efficiency of some of the others.

The correction for the more stringent neutron cuts that I used should probably be about 10%.

If I attribute the above mentioned loss to deadtime in the pulse-height or pulse-shape apparatus, then it is tempting to infer that the backgrounds have a somewhat higher multiplicity (of gammas or muons) than that of the Californium (2-3 neutrons).

The probability of self veto was given as 0.6%, so the probability of veto for a burst of 100 neutrons is 45%, which could constitute a substantial inefficiency.

The probability that a burst of 20 neutrons should have a ring multiplicity of 3 or more, is from 5% to 12%, depending on the exact effect of deadtime. If a few such events occurred, they would not be expected to be detected. Bursts of 125 neutrons have probabilities of from 78% to 99% of being detected. We can conclude that there were probably no events of more than 50 neutrons in the data runs, although calculating the exact confidence levels remains to be done. The excess of double hits in the foreground over the background in Table 1, might be consistent with some smaller bursts, but it is difficult to set an upper limit on bursts of less than 50 neutrons.

Excluding double-hit problems!

I have looked at a selection of the data consisting of about 400K foreground events representing about 56 hours worth of data. The selection also includes a corresponding amount of background and some ^{252}Cf (neutron source) data. No ^{60}Co (energy calibration) data were available. This subset of the data contains the events mentioned in the interim report, and it is unlikely that subsequent review of the entire data set will be able to change any of the conclusions.

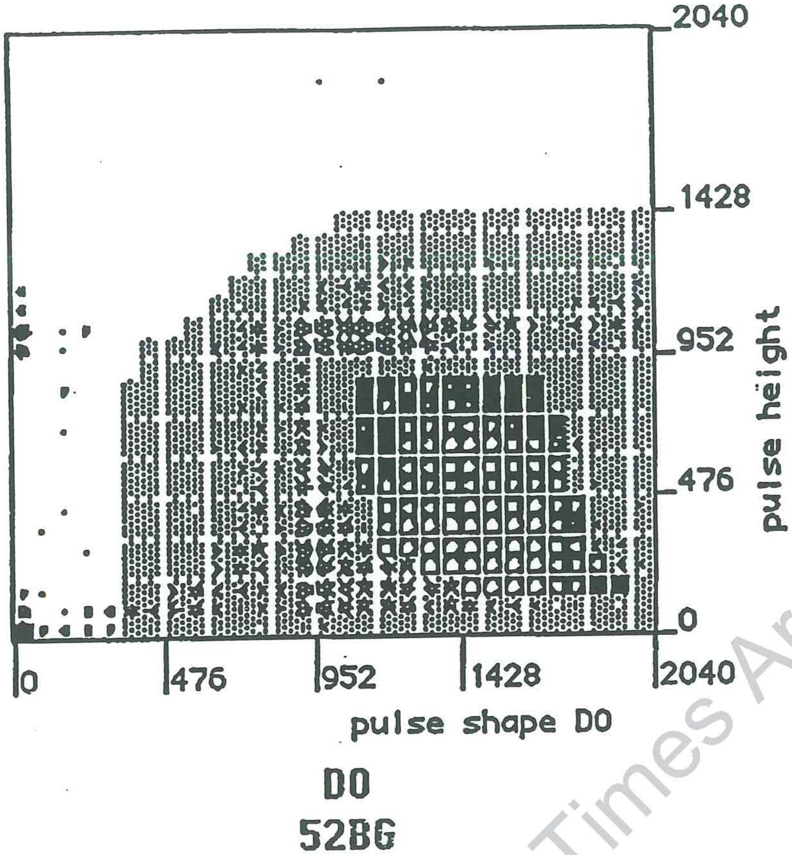
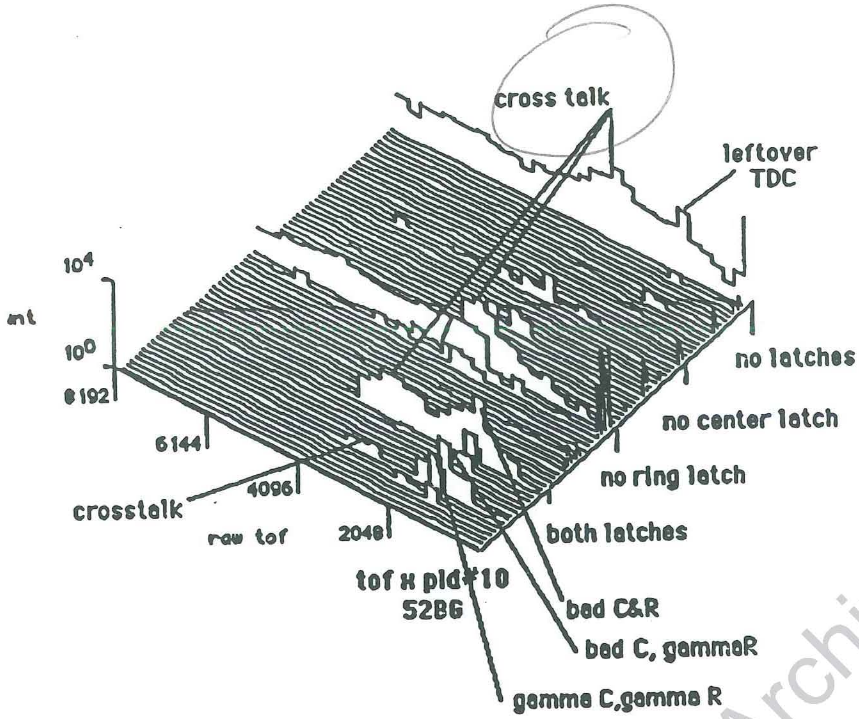


Figure 1. Pulse shape vs. height in D0 for background run 52. Distribution of events within neutron gate indicates they are not spill from gammas.



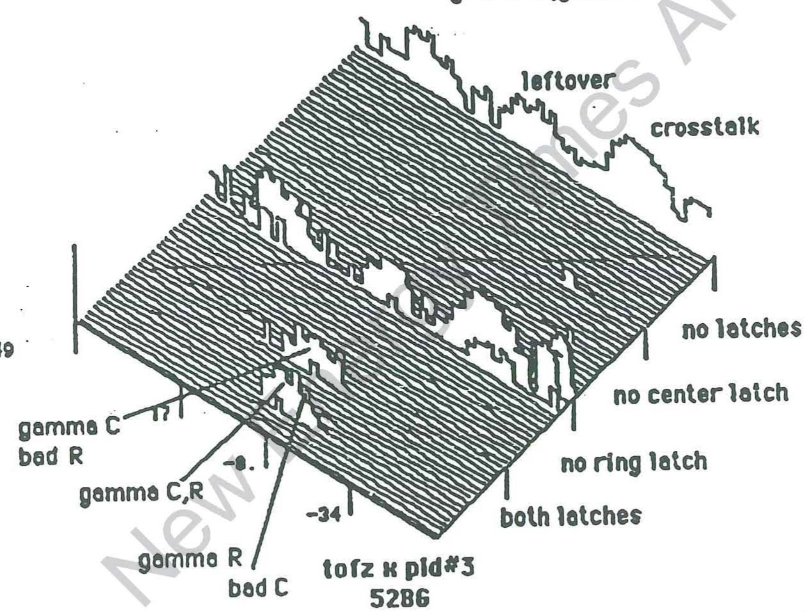


Figure 2. Particle ID vs time of flight for detectors D1 (#6) (@ 2.5ns/chan), and D5 (10) (@256ns./chan) in background run 52. Part. ID is described in the text.

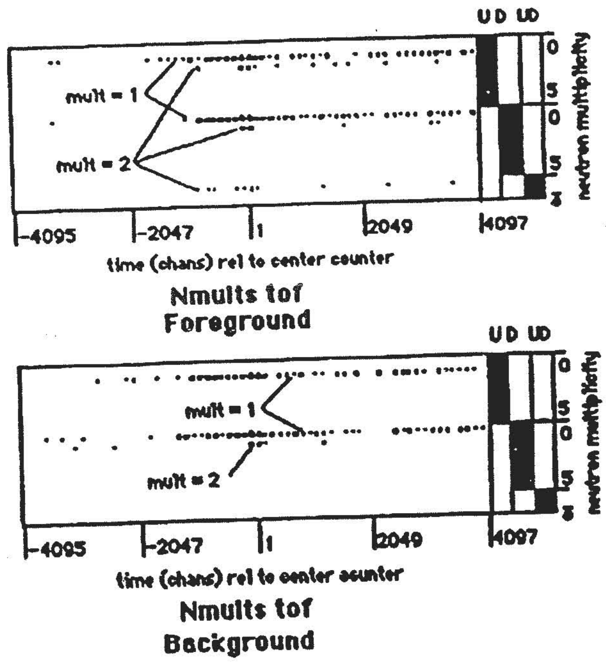


Figure 3. Distribution in time of center-ring coincidences as a function of the neutron multiplicity, for all foreground and all background runs.

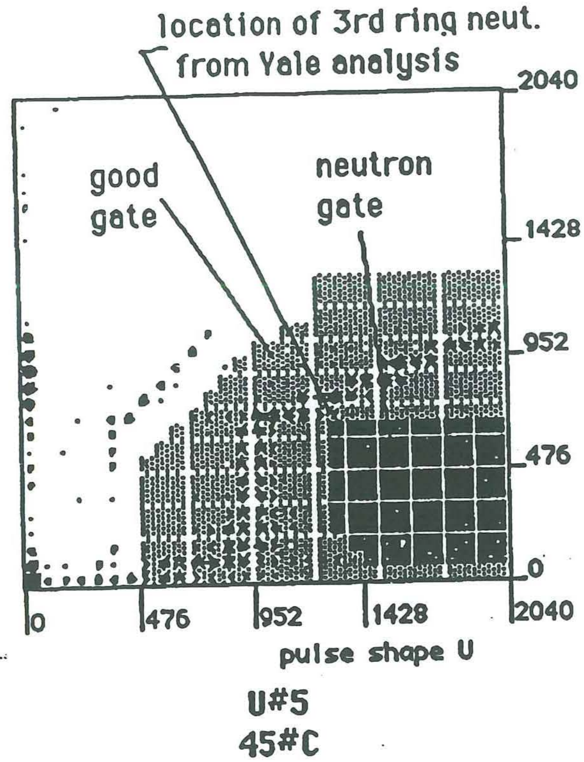


Figure 4. Pulse height spectrum for U5 in data run 45, showing location of the triple-neutron coincidence event which was rejected for having too large a pulse height.

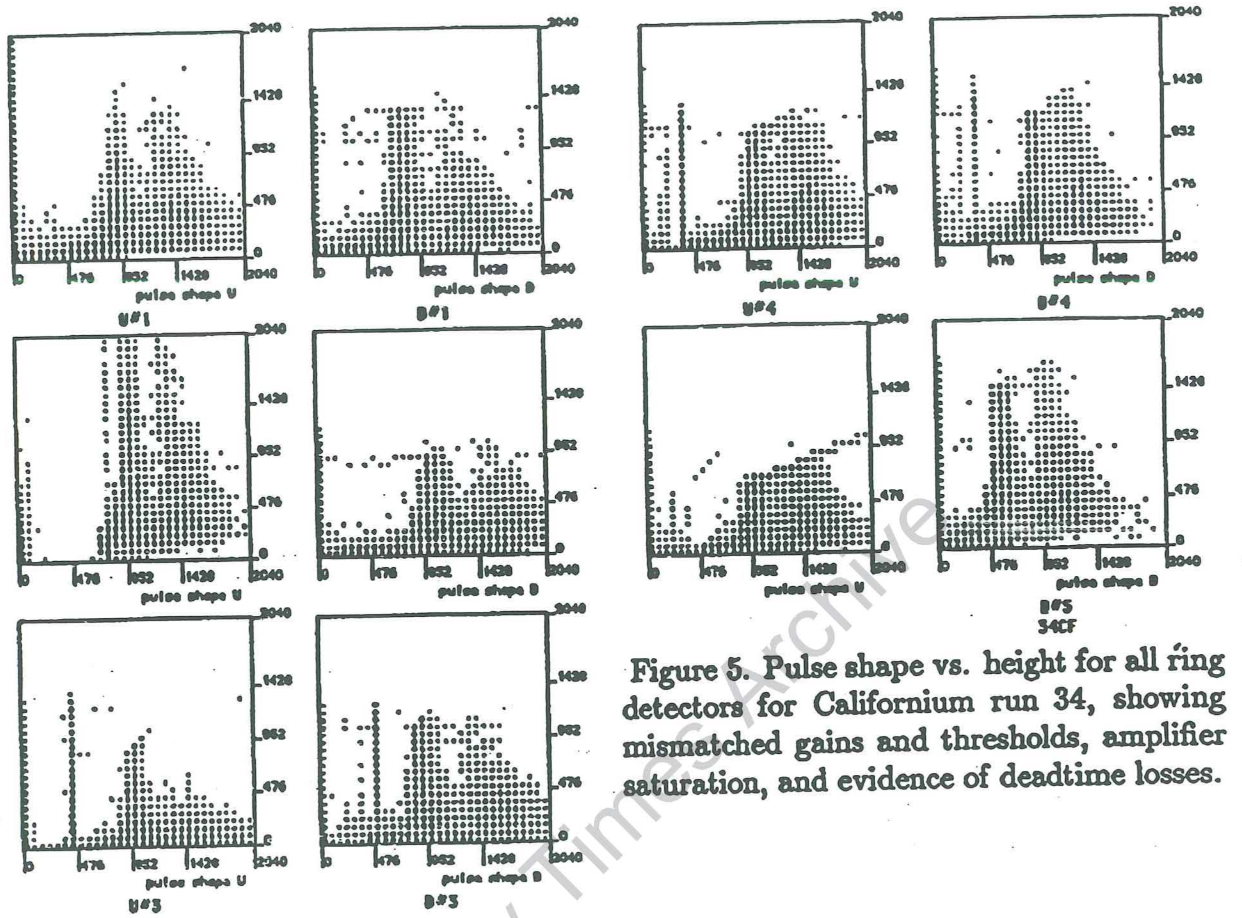


Figure 5. Pulse shape vs. height for all ring detectors for Californium run 34, showing mismatched gains and thresholds, amplifier saturation, and evidence of deadtime losses.

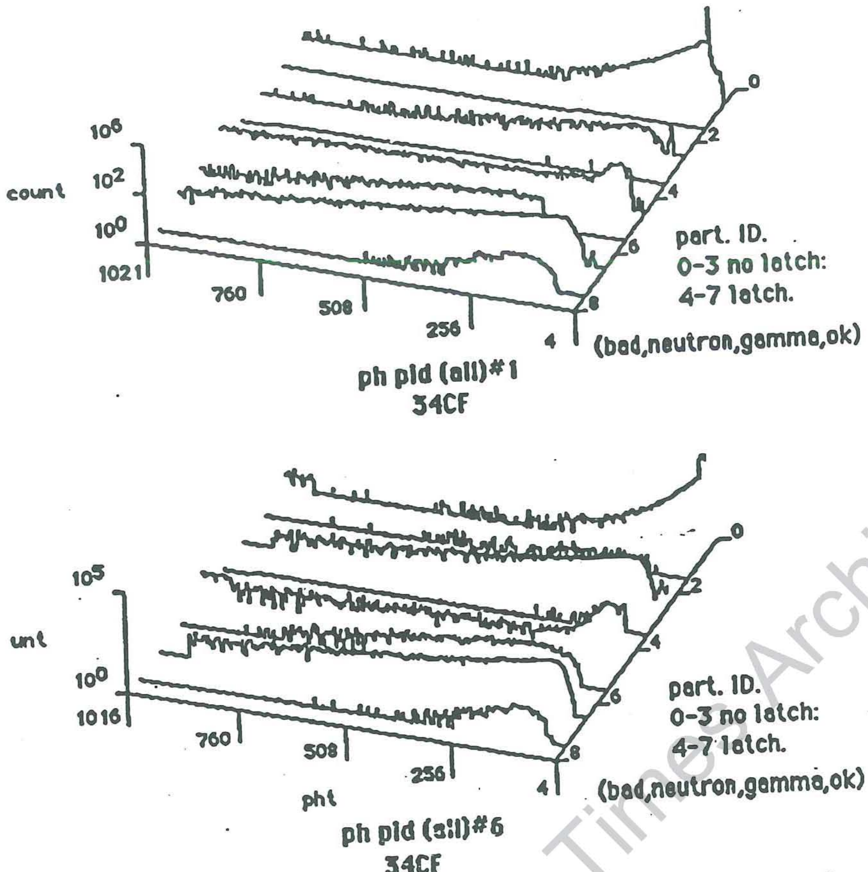


Figure 6. Pulse height vs. particle ID, showing good pedestals for detector U1 (1), and poorly adjusted pedestals for detector D1 (6). Data is from Californium run 34.

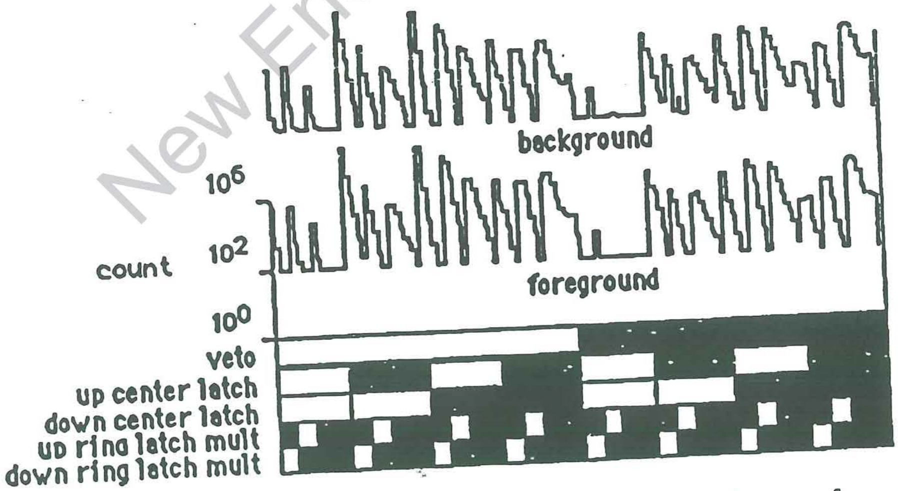


Figure 7. Latch multiplicities for all foreground and background runs. Note events with no latches.

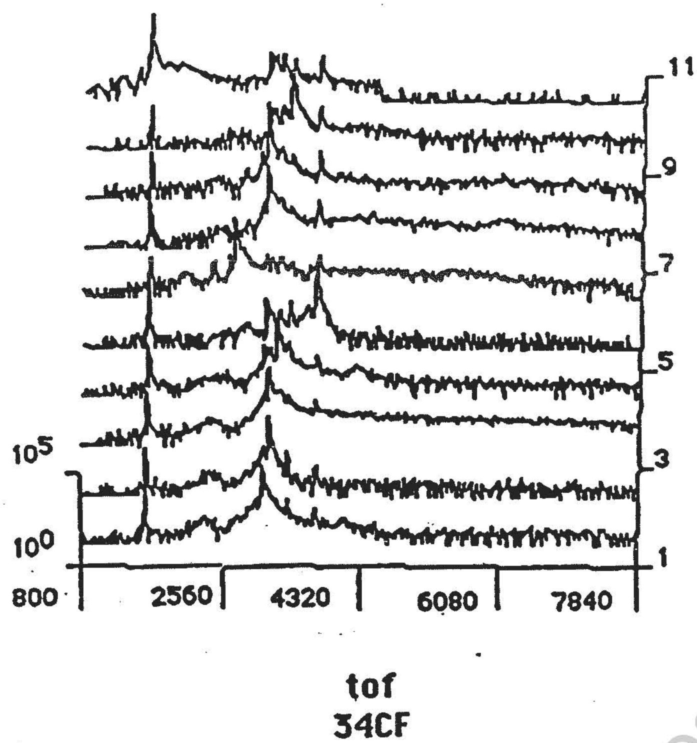


Figure 8. Full scale (10μs.) time of flight vs. detector for Californium run 34. Note nonuniform delays, and crosstalk from detector 10.

Appendix A. Neutron counts as a fraction of total clean events.

First row after run name is total events with both ring and center latch set, with time of flight between channels 0 and 20 (relative to gamma peak), and satisfying the 'ok' event gate on pulse shape/pulse height spectra. Second row is subset whic also satisfy the 'neutron' gate. Third row is ratio of previous numbers.

tlow = 0, thigh = 20

run:	u1	u2	u3	u4	u5	d1	d2	d3	d4	d5	sum	ave
22CF:	00029	00023	00022	00025	00000	00022	00025	00029	00022	00035	00232	
(xx)	00001	00000	00000	00002	00000	00000	00001	00000	00001	00003	00008	
	0.034	0.000	0.000	0.080	0.000	0.000	0.040	0.000	0.045	0.086		0.034
30BG:	00007	00002	00008	00002	00002	00006	00003	00012	00001	00004	00047	
(xx)	00000	00000	00000	00000	00000	00000	00000	00001	00000	00000	00001	
	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.083	0.000	0.000		0.021
31BG:	00009	00020	00012	00010	00019	00020	00014	00023	00006	00019	00152	
(xx)	00000	00000	00000	00000	00000	00000	00000	00000	00001	00000	00001	
	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.167	0.000		0.007
32BG:	00312	00332	00428	00431	00337	00327	00431	00533	00350	00487	03968	
(xx)	00000	00000	00000	00001	00000	00000	00004	00003	00004	00005	00017	
	0.000	0.000	0.000	0.002	0.000	0.000	0.009	0.006	0.011	0.010		0.004
34CF:	09329	08602	10304	06314	11385	13230	11462	12334	08119	16076	107155	
(CF)	01590	01510	00381	01731	02505	00948	01517	02523	01641	02044	16390	
	0.170	0.176	0.037	0.274	0.220	0.072	0.132	0.205	0.202	0.127		0.153
40BG:	00068	00091	00156	00026	00150	00356	00171	00233	00169	00493	01913	
(BG)	00001	00000	00001	00000	00001	00002	00002	00003	00004	00010	00024	
	0.015	0.000	0.006	0.000	0.007	0.006	0.012	0.013	0.024	0.020		0.013
41BG:	00001	00004	00009	00002	00006	00094	00011	00013	00004	00163	00307	
(BG)	00001	00001	00000	00000	00001	00000	00000	00001	00000	00004	00008	
	1.000	0.250	0.000	0.000	0.167	0.000	0.000	0.077	0.000	0.025		0.026
42#A:	00002	00001	00004	00001	00002	00105	00024	00004	00002	00243	00388	
(FG)	00000	00000	00001	00001	00002	00000	00002	00001	00000	00003	00010	
	0.000	0.000	0.250	1.000	1.000	0.000	0.083	0.250	0.000	0.012		0.026
43#A:	00135	00130	00241	00090	00325	00442	00459	00228	00338	00684	03072	
(FG)	00002	00001	00000	00001	00002	00002	00002	00001	00009	00008	00028	
	0.015	0.008	0.000	0.011	0.006	0.005	0.004	0.004	0.027	0.012		0.009
44#B:	00075	00052	00148	00007	00220	00363	00307	00135	00129	00505	01941	
(FG)	00000	00000	00001	00000	00003	00000	00005	00003	00001	00007	00020	
	0.000	0.000	0.007	0.000	0.014	0.000	0.016	0.022	0.008	0.014		0.010
45#C:	00098	00086	00177	00058	00249	00346	00332	00419	00264	00538	02567	
(FG)	00000	00000	00000	00000	00000	00002	00001	00002	00005	00007	00017	
	0.000	0.000	0.000	0.000	0.000	0.006	0.003	0.005	0.019	0.013		0.007
46#D:	00067	00049	00129	00009	00204	00448	00367	00492	00215	00528	02508	
(FG)	00002	00001	00000	00000	00003	00001	00000	00006	00001	00006	00020	
	0.030	0.020	0.000	0.000	0.015	0.002	0.000	0.012	0.005	0.011		0.008
47#D:	00012	00007	00036	00002	00019	00202	00167	00146	00041	00250	00882	
(FG)	00000	00000	00000	00000	00001	00001	00000	00001	00000	00001	00004	
	0.000	0.000	0.000	0.000	0.053	0.005	0.000	0.007	0.000	0.004		0.005
52BG:	00029	00076	00007	00033	00107	00152	00152	00141	00095	00226	01018	
(BG)	00000	00002	00000	00000	00000	00000	00001	00001	00001	00003	00008	
	0.000	0.026	0.000	0.000	0.000	0.000	0.007	0.007	0.011	0.013		0.008
53#G:	00024	00065	00004	00008	00089	00151	00143	00118	00112	00206	00920	
(FG)	00000	00001	00000	00000	00002	00000	00001	00001	00002	00002	00009	
	0.000	0.015	0.000	0.000	0.022	0.000	0.007	0.008	0.018	0.010		0.010
average n fraction:												
	0.012	0.011	0.003	0.008	0.011	0.003	0.007	0.010	0.017	0.013		
						low			high			

type	n	all	n/all
FG:	00108	012278	0.009 +- 0.0008
BG:	00040	003238	0.012 +- 0.0020
CF:	16390	107155	0.153 +- 0.0012
xx:	00748	015172	0.049 +- 0.0018

Appendix B. Probabilities for Ring-detector Multiplicities

burst = 20 neutrons, count=10000 ei=0.008, e0 = 0.10, 10 dets ddtm=0.10

Center:	ignore,		any			sngl			predict
Ring:	any	sngl	only	any	sngl	only	any	sngl	only	(ign,only)

										predict (ign,only)
Probability for exact ring multiplicity										
0	0.1838	0.1873	0.1838	0.1596	0.1629	0.1596	0.1412	0.1441	0.1412	0.1887
1	0.3593	0.3604	0.3558	0.3165	0.3173	0.3132	0.2744	0.2753	0.2715	0.3282
2	0.2839	0.2824	0.2793	0.2502	0.2489	0.2461	0.2200	0.2187	0.2162	0.2440
3	0.1298	0.1282	0.1267	0.1131	0.1117	0.1103	0.1009	0.0995	0.0984	0.1018
4	0.0365	0.0352	0.0350	0.0320	0.0308	0.0306	0.0278	0.0269	0.0267	0.0263
5	0.0061	0.0059	0.0059	0.0058	0.0056	0.0056	0.0053	0.0051	0.0051	0.0044
6	0.0005	0.0005	0.0005	0.0003	0.0003	0.0003	0.0002	0.0002	0.0002	0.0005
7	0.0001	0.0001	0.0001	0.0001	0.0001	0.0001	0.0001	0.0001	0.0001	0.0000
8	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
9	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
10	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
accumulated probability for n or more ring hits										
0	1.0000	1.0000	0.9871	0.8776	0.8776	0.8658	0.7699	0.7699	0.7594	0.8939
1	0.8162	0.8127	0.8033	0.7180	0.7147	0.7062	0.6287	0.6258	0.6182	0.7052
2	0.4569	0.4523	0.4475	0.4015	0.3974	0.3930	0.3543	0.3505	0.3467	0.3771
3	0.1730	0.1699	0.1682	0.1513	0.1485	0.1469	0.1343	0.1318	0.1305	0.1331
4	0.0432	0.0417	0.0415	0.0382	0.0368	0.0366	0.0334	0.0323	0.0321	0.0313
5	0.0067	0.0065	0.0065	0.0062	0.0060	0.0060	0.0056	0.0054	0.0054	0.0049
6	0.0006	0.0006	0.0006	0.0004	0.0004	0.0004	0.0003	0.0003	0.0003	0.0005
7	0.0001	0.0001	0.0001	0.0001	0.0001	0.0001	0.0001	0.0001	0.0001	0.0000
8	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
9	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
10	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Expected Multiplicity:	1.4967	1.4838	1.4869	1.4992	1.4858	1.4890	1.5024	1.4888	1.4924	1.4007

burst = 20 neutrons, count=10000 ei=0.005, e0 = 0.10, 10 dets ddtm=1.00

Center:	ignore,		any			sngl			predict
Ring:	any	sngl	only	any	sngl	only	any	sngl	only	(ign,only)

										predict (ign,only)
Probability for exact ring multiplicity										
0	0.3487	0.3712	0.3487	0.3043	0.3240	0.3043	0.0913	0.0976	0.0913	0.3585
1	0.4017	0.3959	0.3794	0.3537	0.3489	0.3342	0.1055	0.1041	0.0993	0.3774
2	0.1899	0.1794	0.1735	0.1669	0.1579	0.1523	0.0520	0.0490	0.0472	0.1698
3	0.0507	0.0458	0.0445	0.0442	0.0395	0.0383	0.0138	0.0125	0.0119	0.0429
4	0.0083	0.0071	0.0070	0.0074	0.0063	0.0062	0.0028	0.0023	0.0022	0.0067
5	0.0006	0.0005	0.0005	0.0006	0.0005	0.0005	0.0002	0.0001	0.0001	0.0007
6	0.0001	0.0001	0.0001	0.0001	0.0001	0.0001	0.0000	0.0000	0.0000	0.0000
7	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
8	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
9	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
10	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
accumulated probability for n or more ring hits										
0	1.0000	1.0000	0.9537	0.8772	0.8772	0.8359	0.2656	0.2656	0.2520	0.9560
1	0.6513	0.6288	0.6050	0.5729	0.5532	0.5316	0.1743	0.1680	0.1607	0.5975
2	0.2496	0.2329	0.2256	0.2192	0.2043	0.1974	0.0688	0.0639	0.0614	0.2202
3	0.0597	0.0535	0.0521	0.0523	0.0464	0.0451	0.0168	0.0149	0.0142	0.0503
4	0.0090	0.0077	0.0076	0.0081	0.0069	0.0068	0.0030	0.0024	0.0023	0.0074
5	0.0007	0.0006	0.0006	0.0007	0.0006	0.0006	0.0002	0.0001	0.0001	0.0007
6	0.0001	0.0001	0.0001	0.0001	0.0001	0.0001	0.0000	0.0000	0.0000	0.0000
7	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
8	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
9	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
10	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Expected Multiplicity:	0.9704	0.9236	0.9343	0.9728	0.9251	0.9350	0.9906	0.9386	0.9472	0.9166

burst = 20 neutrons, count=10000 ei=0.008, e0 = 0.10, 10 dets ddtm=1.00

	Center:ignore,			any			sngl			predict
Ring:	any	sngl	only	any	sngl	only	any	sngl	only	(ign,only)
Probability for exact ring multiplicity										
0:	0.1838	0.2159	0.1838	0.1596	0.1879	0.1596	0.0461	0.0545	0.0461	0.1887
1:	0.3593	0.3705	0.3286	0.3165	0.3257	0.2892	0.0938	0.0972	0.0855	0.3282
2:	0.2839	0.2673	0.2418	0.2502	0.2361	0.2138	0.0756	0.0713	0.0640	0.2440
3:	0.1298	0.1134	0.1042	0.1131	0.0988	0.0906	0.0332	0.0279	0.0262	0.1018
4:	0.0365	0.0278	0.0266	0.0320	0.0245	0.0233	0.0090	0.0074	0.0070	0.0263
5:	0.0061	0.0047	0.0045	0.0058	0.0043	0.0042	0.0017	0.0011	0.0011	0.0044
6:	0.0005	0.0004	0.0003	0.0003	0.0003	0.0002	0.0001	0.0002	0.0001	0.0005
7:	0.0001	0.0000	0.0000	0.0001	0.0000	0.0000	0.0001	0.0000	0.0000	0.0000
8:	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
9:	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
10:	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
accumulated probability for n or more ring hits										
0:	1.0000	1.0000	0.8898	0.8776	0.8776	0.7809	0.2596	0.2596	0.2300	0.8939
1:	0.8162	0.7841	0.7060	0.7180	0.6897	0.6213	0.2135	0.2051	0.1839	0.7052
2:	0.4569	0.4136	0.3774	0.4015	0.3640	0.3321	0.1197	0.1079	0.0984	0.3771
3:	0.1730	0.1463	0.1356	0.1513	0.1279	0.1183	0.0441	0.0366	0.0344	0.1331
4:	0.0432	0.0329	0.0314	0.0382	0.0291	0.0277	0.0109	0.0087	0.0082	0.0313
5:	0.0067	0.0051	0.0048	0.0062	0.0046	0.0044	0.0019	0.0013	0.0012	0.0049
6:	0.0006	0.0004	0.0003	0.0004	0.0003	0.0002	0.0002	0.0002	0.0001	0.0005
7:	0.0001	0.0000	0.0000	0.0001	0.0000	0.0000	0.0001	0.0000	0.0000	0.0000
8:	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
9:	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
10:	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Expected Multiplicity:	1.4967	1.3824	1.4110	1.4992	1.3851	1.4138	1.5039	1.3860	1.4183	1.4007

burst = 50 neutrons, count=10000 ei=0.008, e0 = 0.10, 10 dets ddtm=0.00

	Center:ignore,			any			sngl			predict
Ring:	any	sngl	only	any	sngl	only	any	sngl	only	(ign,only)
Probability for exact ring multiplicity										
0:	0.0167	0.0167	0.0167	0.0167	0.0167	0.0167	0.0167	0.0167	0.0167	0.0155
1:	0.0788	0.0788	0.0788	0.0785	0.0785	0.0785	0.0785	0.0785	0.0785	0.0672
2:	0.1986	0.1986	0.1986	0.1979	0.1979	0.1979	0.1979	0.1979	0.1979	0.1289
3:	0.2673	0.2673	0.2673	0.2663	0.2663	0.2663	0.2663	0.2663	0.2663	0.1435
4:	0.2367	0.2367	0.2367	0.2356	0.2356	0.2356	0.2356	0.2356	0.2356	0.1026
5:	0.1356	0.1356	0.1356	0.1348	0.1348	0.1348	0.1348	0.1348	0.1348	0.0493
6:	0.0522	0.0522	0.0522	0.0516	0.0516	0.0516	0.0516	0.0516	0.0516	0.0161
7:	0.0119	0.0119	0.0119	0.0118	0.0118	0.0118	0.0118	0.0118	0.0118	0.0035
8:	0.0020	0.0020	0.0020	0.0020	0.0020	0.0020	0.0020	0.0020	0.0020	0.0005
9:	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002	0.0000
10:	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
accumulated probability for n or more ring hits										
0:	1.0000	1.0000	1.0000	0.9954	0.9954	0.9954	0.9954	0.9954	0.9954	0.5272
1:	0.9833	0.9833	0.9833	0.9787	0.9787	0.9787	0.9787	0.9787	0.9787	0.5117
2:	0.9045	0.9045	0.9045	0.9002	0.9002	0.9002	0.9002	0.9002	0.9002	0.4445
3:	0.7059	0.7059	0.7059	0.7023	0.7023	0.7023	0.7023	0.7023	0.7023	0.3155
4:	0.4386	0.4386	0.4386	0.4360	0.4360	0.4360	0.4360	0.4360	0.4360	0.1720
5:	0.2019	0.2019	0.2019	0.2004	0.2004	0.2004	0.2004	0.2004	0.2004	0.0694
6:	0.0663	0.0663	0.0663	0.0656	0.0656	0.0656	0.0656	0.0656	0.0656	0.0201
7:	0.0141	0.0141	0.0141	0.0140	0.0140	0.0140	0.0140	0.0140	0.0140	0.0040
8:	0.0022	0.0022	0.0022	0.0022	0.0022	0.0022	0.0022	0.0022	0.0022	0.0005
9:	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002	0.0000
10:	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Expected Multiplicity:	3.3170	3.3170	3.3170	3.3148	3.3148	3.3148	3.3148	3.3148	3.3148	2.9171

burst = 50 neutrons, count=10000 ei=0.005, e0 = 0.10, 10 dets ddtm=0.10

Center:	ignore,			any			sngl			
Ring:	any	sngl	only	any	sngl	only	any	sngl	only	predict (ign,only)
Probability for exact ring multiplicity										
0:	0.0779	0.0820	0.0779	0.0772	0.0813	0.0772	0.0507	0.0532	0.0507	0.0769
1:	0.2298	0.2345	0.2259	0.2284	0.2331	0.2245	0.1546	0.1582	0.1523	0.2025
2:	0.3063	0.3062	0.2978	0.3046	0.3044	0.2961	0.2037	0.2032	0.1977	0.2350
3:	0.2277	0.2238	0.2190	0.2269	0.2231	0.2183	0.1533	0.1511	0.1477	0.1583
4:	0.1141	0.1108	0.1094	0.1136	0.1103	0.1089	0.0751	0.0727	0.0717	0.0685
5:	0.0344	0.0336	0.0330	0.0341	0.0333	0.0327	0.0228	0.0221	0.0218	0.0199
6:	0.0078	0.0072	0.0071	0.0077	0.0071	0.0070	0.0045	0.0043	0.0042	0.0039
7:	0.0018	0.0017	0.0017	0.0018	0.0017	0.0017	0.0015	0.0014	0.0014	0.0005
8:	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002	0.0000
9:	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
10:	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
accumulated probability for n or more ring hits										
0:	1.0000	1.0000	0.9720	0.9945	0.9945	0.9666	0.6664	0.6664	0.6477	0.7657
1:	0.9221	0.9180	0.8941	0.9173	0.9132	0.8894	0.6157	0.6132	0.5970	0.6887
2:	0.6923	0.6835	0.6682	0.6889	0.6801	0.6649	0.4611	0.4550	0.4447	0.4862
3:	0.3860	0.3773	0.3704	0.3843	0.3757	0.3688	0.2574	0.2518	0.2470	0.2512
4:	0.1583	0.1535	0.1514	0.1574	0.1526	0.1505	0.1041	0.1007	0.0993	0.0929
5:	0.0442	0.0427	0.0420	0.0438	0.0423	0.0416	0.0290	0.0280	0.0276	0.0244
6:	0.0098	0.0091	0.0090	0.0097	0.0090	0.0089	0.0062	0.0059	0.0058	0.0045
7:	0.0020	0.0019	0.0019	0.0020	0.0019	0.0019	0.0017	0.0016	0.0016	0.0006
8:	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002	0.0000
9:	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
10:	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Expected Multiplicity:										
	2.2149	2.1862	2.1988	2.2158	2.1870	2.1997	2.2140	2.1855	2.1973	2.0226

burst = 50 neutrons, count=10000 ei=0.008, e0 = 0.10, 10 dets ddtm=0.10

Center:	ignore,			any			sngl			
Ring:	any	sngl	only	any	sngl	only	any	sngl	only	predict (ign,only)
Probability for exact ring multiplicity										
0:	0.0167	0.0193	0.0167	0.0167	0.0193	0.0167	0.0111	0.0125	0.0111	0.0155
1:	0.0788	0.0845	0.0763	0.0785	0.0842	0.0760	0.0523	0.0563	0.0510	0.0672
2:	0.1986	0.2083	0.1906	0.1979	0.2075	0.1899	0.1311	0.1379	0.1259	0.1289
3:	0.2673	0.2668	0.2498	0.2663	0.2659	0.2489	0.1807	0.1799	0.1687	0.1435
4:	0.2367	0.2334	0.2198	0.2356	0.2323	0.2187	0.1608	0.1591	0.1499	0.1026
5:	0.1356	0.1286	0.1221	0.1348	0.1278	0.1213	0.0910	0.0862	0.0818	0.0493
6:	0.0522	0.0464	0.0451	0.0516	0.0458	0.0445	0.0339	0.0303	0.0290	0.0161
7:	0.0119	0.0106	0.0105	0.0118	0.0105	0.0104	0.0083	0.0070	0.0070	0.0035
8:	0.0020	0.0019	0.0019	0.0020	0.0019	0.0019	0.0011	0.0011	0.0011	0.0005
9:	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002	0.0001	0.0001	0.0001	0.0000
10:	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
accumulated probability for n or more ring hits										
0:	1.0000	1.0000	0.9330	0.9954	0.9954	0.9285	0.6704	0.6704	0.6256	0.5272
1:	0.9833	0.9807	0.9163	0.9787	0.9761	0.9118	0.6593	0.6579	0.6145	0.5117
2:	0.9045	0.8962	0.8400	0.9002	0.8919	0.8358	0.6070	0.6016	0.5635	0.4445
3:	0.7059	0.6879	0.6494	0.7023	0.6844	0.6459	0.4759	0.4637	0.4376	0.3155
4:	0.4386	0.4211	0.3996	0.4360	0.4185	0.3970	0.2952	0.2838	0.2689	0.1720
5:	0.2019	0.1877	0.1798	0.2004	0.1862	0.1783	0.1344	0.1247	0.1190	0.0694
6:	0.0663	0.0591	0.0577	0.0656	0.0584	0.0570	0.0434	0.0385	0.0372	0.0201
7:	0.0141	0.0127	0.0126	0.0140	0.0126	0.0125	0.0095	0.0082	0.0082	0.0040
8:	0.0022	0.0021	0.0021	0.0022	0.0021	0.0021	0.0012	0.0012	0.0012	0.0005
9:	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002	0.0001	0.0001	0.0001	0.0000
10:	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Expected Multiplicity:										
	3.3170	3.2477	3.2773	3.3148	3.2453	3.2747	3.3204	3.2513	3.2772	2.9171

burst = 50 neutrons, count=10000 ei=0.005, e0 = 0.10, 10 dets ddtm=1.00

Center:ignore, Ring:	any	sngl	only	any: any	any: sngl	any: only	sngl: any	sngl: sngl	sngl: only	predict (ign,only)
Probability for exact ring multiplicity										
0:	0.0779	0.1108	0.0779	0.0772	0.1100	0.0772	0.0023	0.0033	0.0023	0.0769
1:	0.2298	0.2759	0.2013	0.2284	0.2744	0.2000	0.0075	0.0094	0.0066	0.2025
2:	0.3063	0.3056	0.2356	0.3046	0.3036	0.2341	0.0100	0.0095	0.0076	0.2350
3:	0.2277	0.1952	0.1572	0.2269	0.1947	0.1569	0.0067	0.0058	0.0044	0.1583
4:	0.1141	0.0839	0.0716	0.1136	0.0835	0.0713	0.0034	0.0023	0.0020	0.0685
5:	0.0344	0.0221	0.0194	0.0341	0.0219	0.0192	0.0007	0.0003	0.0003	0.0199
6:	0.0078	0.0052	0.0045	0.0077	0.0051	0.0044	0.0000	0.0000	0.0000	0.0039
7:	0.0018	0.0013	0.0011	0.0018	0.0013	0.0011	0.0000	0.0000	0.0000	0.0005
8:	0.0002	0.0000	0.0000	0.0002	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
9:	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
10:	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
accumulated probability for n or more ring hits										
0:	1.0000	1.0000	0.7686	0.9945	0.9945	0.7642	0.0306	0.0306	0.0232	0.7657
1:	0.9221	0.8892	0.6907	0.9173	0.8845	0.6870	0.0283	0.0273	0.0209	0.6887
2:	0.6923	0.6133	0.4894	0.6889	0.6101	0.4870	0.0208	0.0179	0.0143	0.4862
3:	0.3860	0.3077	0.2538	0.3843	0.3065	0.2529	0.0108	0.0084	0.0067	0.2512
4:	0.1583	0.1125	0.0966	0.1574	0.1118	0.0960	0.0041	0.0026	0.0023	0.0929
5:	0.0442	0.0286	0.0250	0.0438	0.0283	0.0247	0.0007	0.0003	0.0003	0.0244
6:	0.0098	0.0065	0.0056	0.0097	0.0064	0.0055	0.0000	0.0000	0.0000	0.0045
7:	0.0020	0.0013	0.0011	0.0020	0.0013	0.0011	0.0000	0.0000	0.0000	0.0006
8:	0.0002	0.0000	0.0000	0.0002	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
9:	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
10:	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Expected Multiplicity:	2.2149	1.9591	2.0325	2.2158	1.9597	2.0338	2.1144	1.8464	1.9181	2.0226

burst = 125 neutrons, count=10000 ei=0.008, e0 = 0.10, 10 dets ddtm=0.00

Center:ignore, Ring:	any	sngl	only	any: any	any: sngl	any: only	sngl: any	sngl: sngl	sngl: only	predict (ign,only)
Probability for exact ring multiplicity										
0:	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
1:	0.0003	0.0003	0.0003	0.0003	0.0003	0.0003	0.0003	0.0003	0.0003	0.0003
2:	0.0042	0.0042	0.0042	0.0042	0.0042	0.0042	0.0042	0.0042	0.0042	0.0016
3:	0.0255	0.0255	0.0255	0.0255	0.0255	0.0255	0.0255	0.0255	0.0255	0.0045
4:	0.0785	0.0785	0.0785	0.0785	0.0785	0.0785	0.0785	0.0785	0.0785	0.0083
5:	0.1736	0.1736	0.1736	0.1736	0.1736	0.1736	0.1736	0.1736	0.1736	0.0105
6:	0.2520	0.2520	0.2520	0.2520	0.2520	0.2520	0.2520	0.2520	0.2520	0.0091
7:	0.2481	0.2481	0.2481	0.2481	0.2481	0.2481	0.2481	0.2481	0.2481	0.0054
8:	0.1516	0.1516	0.1516	0.1516	0.1516	0.1516	0.1516	0.1516	0.1516	0.0021
9:	0.0559	0.0559	0.0559	0.0559	0.0559	0.0559	0.0559	0.0559	0.0559	0.0005
10:	0.0103	0.0103	0.0103	0.0103	0.0103	0.0103	0.0103	0.0103	0.0103	0.0000
accumulated probability for n or more ring hits										
0:	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000	0.0423
1:	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000	0.0423
2:	0.9997	0.9997	0.9997	0.9997	0.9997	0.9997	0.9997	0.9997	0.9997	0.0419
3:	0.9955	0.9955	0.9955	0.9955	0.9955	0.9955	0.9955	0.9955	0.9955	0.0404
4:	0.9700	0.9700	0.9700	0.9700	0.9700	0.9700	0.9700	0.9700	0.9700	0.0359
5:	0.8915	0.8915	0.8915	0.8915	0.8915	0.8915	0.8915	0.8915	0.8915	0.0276
6:	0.7179	0.7179	0.7179	0.7179	0.7179	0.7179	0.7179	0.7179	0.7179	0.0171
7:	0.4659	0.4659	0.4659	0.4659	0.4659	0.4659	0.4659	0.4659	0.4659	0.0080
8:	0.2178	0.2178	0.2178	0.2178	0.2178	0.2178	0.2178	0.2178	0.2178	0.0026
9:	0.0662	0.0662	0.0662	0.0662	0.0662	0.0662	0.0662	0.0662	0.0662	0.0005
10:	0.0103	0.0103	0.0103	0.0103	0.0103	0.0103	0.0103	0.0103	0.0103	0.0000
Expected Multiplicity:	6.3348	6.3348	6.3348	6.3348	6.3348	6.3348	6.3348	6.3348	6.3348	5.1144

burst = 125 neutrons, count=10000 ei=0.008, e0 = 0.10, 10 dets ddtm=0.10

Center:	ignore,			any			sngl			
Ring:	any	sngl	only	any	sngl	only	any	sngl	only	predict (ign,only)
Probability for exact ring multiplicity										
0:	0.0000	0.0001	0.0000	0.0000	0.0001	0.0000	0.0000	0.0001	0.0000	0.0000
1:	0.0003	0.0013	0.0003	0.0003	0.0013	0.0003	0.0001	0.0004	0.0001	0.0003
2:	0.0042	0.0097	0.0032	0.0042	0.0097	0.0032	0.0014	0.0032	0.0010	0.0016
3:	0.0255	0.0416	0.0211	0.0255	0.0416	0.0211	0.0074	0.0126	0.0061	0.0045
4:	0.0785	0.1138	0.0611	0.0785	0.1138	0.0611	0.0264	0.0361	0.0207	0.0083
5:	0.1736	0.2053	0.1279	0.1736	0.2053	0.1279	0.0543	0.0640	0.0400	0.0105
6:	0.2520	0.2508	0.1764	0.2520	0.2508	0.1764	0.0796	0.0784	0.0570	0.0091
7:	0.2481	0.2173	0.1685	0.2481	0.2173	0.1685	0.0765	0.0699	0.0538	0.0054
8:	0.1516	0.1166	0.0982	0.1516	0.1166	0.0982	0.0472	0.0352	0.0299	0.0021
9:	0.0559	0.0374	0.0341	0.0559	0.0374	0.0341	0.0191	0.0130	0.0122	0.0005
10:	0.0103	0.0061	0.0061	0.0103	0.0061	0.0061	0.0032	0.0023	0.0023	0.0000
accumulated probability for n or more ring hits										
0:	1.0000	1.0000	0.6969	1.0000	1.0000	0.6969	0.3152	0.3152	0.2231	0.0423
1:	1.0000	0.9999	0.6969	1.0000	0.9999	0.6969	0.3152	0.3151	0.2231	0.0423
2:	0.9997	0.9986	0.6966	0.9997	0.9986	0.6966	0.3151	0.3147	0.2230	0.0419
3:	0.9955	0.9889	0.6934	0.9955	0.9889	0.6934	0.3137	0.3115	0.2220	0.0404
4:	0.9700	0.9473	0.6723	0.9700	0.9473	0.6723	0.3063	0.2989	0.2159	0.0359
5:	0.8915	0.8335	0.6112	0.8915	0.8335	0.6112	0.2799	0.2628	0.1952	0.0276
6:	0.7179	0.6282	0.4833	0.7179	0.6282	0.4833	0.2256	0.1988	0.1552	0.0171
7:	0.4659	0.3774	0.3069	0.4659	0.3774	0.3069	0.1460	0.1204	0.0982	0.0080
8:	0.2178	0.1601	0.1384	0.2178	0.1601	0.1384	0.0695	0.0505	0.0444	0.0026
9:	0.0662	0.0435	0.0402	0.0662	0.0435	0.0402	0.0223	0.0153	0.0145	0.0005
10:	0.0103	0.0061	0.0061	0.0103	0.0061	0.0061	0.0032	0.0023	0.0023	0.0000
Expected Multiplicity:										
	6.3348	5.9835	6.2352	6.3348	5.9835	6.2352	6.3350	5.9971	6.2474	5.1144

burst = 125 neutrons, count=10000 ei=0.008, e0 = 0.10, 10 dets ddtm=1.00

Center:	ignore,			any			sngl			
Ring:	any	sngl	only	any	sngl	only	any	sngl	only	predict (ign,only)
Probability for exact ring multiplicity										
0:	0.0000	0.0088	0.0000	0.0000	0.0088	0.0000	0.0000	0.0000	0.0000	0.0000
1:	0.0003	0.0577	0.0003	0.0003	0.0577	0.0003	0.0000	0.0000	0.0000	0.0003
2:	0.0042	0.1548	0.0017	0.0042	0.1548	0.0017	0.0000	0.0000	0.0000	0.0016
3:	0.0255	0.2442	0.0034	0.0255	0.2442	0.0034	0.0000	0.0000	0.0000	0.0045
4:	0.0785	0.2417	0.0074	0.0785	0.2417	0.0074	0.0000	0.0001	0.0000	0.0083
5:	0.1736	0.1701	0.0108	0.1736	0.1701	0.0108	0.0000	0.0000	0.0000	0.0105
6:	0.2520	0.0894	0.0090	0.2520	0.0894	0.0090	0.0000	0.0000	0.0000	0.0091
7:	0.2481	0.0271	0.0042	0.2481	0.0271	0.0042	0.0000	0.0000	0.0000	0.0054
8:	0.1516	0.0053	0.0016	0.1516	0.0053	0.0016	0.0001	0.0000	0.0000	0.0021
9:	0.0559	0.0009	0.0004	0.0559	0.0009	0.0004	0.0000	0.0000	0.0000	0.0005
10:	0.0103	0.0000	0.0000	0.0103	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
accumulated probability for n or more ring hits										
0:	1.0000	1.0000	0.0388	1.0000	1.0000	0.0388	0.0001	0.0001	0.0000	0.0423
1:	1.0000	0.9912	0.0388	1.0000	0.9912	0.0388	0.0001	0.0001	0.0000	0.0423
2:	0.9997	0.9335	0.0385	0.9997	0.9335	0.0385	0.0001	0.0001	0.0000	0.0419
3:	0.9955	0.7787	0.0368	0.9955	0.7787	0.0368	0.0001	0.0001	0.0000	0.0404
4:	0.9700	0.5345	0.0334	0.9700	0.5345	0.0334	0.0001	0.0001	0.0000	0.0359
5:	0.8915	0.2928	0.0260	0.8915	0.2928	0.0260	0.0001	0.0000	0.0000	0.0276
6:	0.7179	0.1227	0.0152	0.7179	0.1227	0.0152	0.0001	0.0000	0.0000	0.0171
7:	0.4659	0.0333	0.0062	0.4659	0.0333	0.0062	0.0001	0.0000	0.0000	0.0080
8:	0.2178	0.0062	0.0020	0.2178	0.0062	0.0020	0.0001	0.0000	0.0000	0.0026
9:	0.0662	0.0009	0.0004	0.0662	0.0009	0.0004	0.0000	0.0000	0.0000	0.0005
10:	0.0103	0.0000	0.0000	0.0103	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Expected Multiplicity:										
	6.3348	3.6938	5.0851	6.3348	3.6938	5.0851	8.0000	4.0000	0.0000	5.1144

Appendix C. Inferred Electronic Efficiencies.

Denominator(sum) is the sum of those events with a latch for the given detector and its corresponding center detector and time of flight between 0 and 20 channels, (relative to gamma peak). Numerator(ok) is subset which also satisfied the 'ok' ev gate on pulse shape/pulse height spectra. D1 is not included in the averages becaus of the presence of obviously spurious events in its denominator.

run:	u1	u2	u3	u4	u5	d1	d2	d3	d4	d5		ave*,	sum*,ok* (*w/o	
22CF:	0.66	0.80	0.42	0.56	0.00	0.49	0.56	0.56	0.54	0.59	->	0.521091	000432	000247
23??:	0.87	0.95	0.68	0.75	0.97	0.62	0.72	0.74	0.85	0.76	->	0.809598	014939	011685
30BG:	0.89	1.00	0.64	0.38	1.00	0.17	0.50	1.00	1.00	0.83	->	0.804453	000067	000051
31BG:	0.67	0.81	0.75	0.58	0.95	0.08	0.41	0.55	0.87	0.48	->	0.673284	000257	000160
32BG:	0.72	0.87	0.66	0.72	0.93	0.08	0.47	0.53	0.83	0.55	->	0.697263	006159	003997
34CF:	0.86	0.94	0.68	0.77	0.96	0.63	0.73	0.74	0.86	0.77	->	0.812243	184127	147378
35PB:	0.47	0.70	0.65	0.64	0.89	0.04	0.34	0.35	0.87	0.46	->	0.596772	001154	000581
36PB:	0.25	0.65	0.64	0.40	0.97	0.03	0.35	0.33	0.81	0.48	->	0.541490	000401	000199
40BG:	0.33	0.53	0.62	0.44	0.86	0.04	0.27	0.34	0.85	0.46	->	0.522281	006604	003010
41BG:	0.30	0.53	0.62	0.61	0.86	0.03	0.31	0.34	0.86	0.44	->	0.540300	002681	001236
42#A:	0.35	0.53	0.64	0.59	0.88	0.03	0.33	0.34	0.82	0.40	->	0.540387	004750	002167
43#A:	0.30	0.53	0.61	0.58	0.88	0.03	0.30	0.36	0.84	0.44	->	0.538271	009573	004418
44#B:	0.29	0.56	0.62	0.43	0.89	0.03	0.31	0.36	0.84	0.45	->	0.527964	006969	003244
45#C:	0.28	0.54	0.62	0.63	0.90	0.04	0.28	0.38	0.83	0.43	->	0.541859	007110	003256
46#D:	0.25	0.47	0.62	0.48	0.87	0.03	0.25	0.33	0.80	0.45	->	0.503666	007735	003294
47#D:	0.23	0.51	0.61	0.58	0.87	0.03	0.30	0.34	0.85	0.45	->	0.527601	003945	001793
52BG:	0.28	0.60	0.59	0.62	0.86	0.04	0.29	0.29	0.85	0.46	->	0.536787	002752	001269
53#G:	0.24	0.52	0.61	0.51	0.89	0.03	0.29	0.33	0.88	0.45	->	0.525211	002639	001237

To: Yale/BNL/BYU collaboration
From: S. E. Jones
Re: Analysis of Yale data from August run
Date: 26 December 1989/revised 3 January 1990

Dear Colleagues:

We set out in the collaboration to search for neutron bursts of the type reported by Menlove et al. of the LANL/BYU collaboration, and specifically to determine the time structure of neutron bursts if any were found. Since receiving the data tapes on October 26, the day before the Interim Report (IR) was submitted to the DOE/ERAB committee on cold fusion, we have found in the data and in the IR a number of items to report to the collaboration. The analysis was done with the help of Stuart Taylor, Russell Hunter and Alan Anderson. Al has worked with me on analysis of neutron data from muon catalysis experiments for years, including interpreting information from liquid-scintillator counters as also used at Yale, and has written up a separate report which is attached. Notice that the term "TDC" is used for "time to digital conversion" though at Yale the TDC was built up from TAC's rather than with the use of TDC units per se.

1. The two central counters have the highest efficiency (reported in IR at approx. 10% each), yet these counters were not timed relative to each other (as I learned in October from Kelvin). Why not? These counters give us the best chance of detecting neutrons and since we are after time information, the lack of this information for the most efficient counters should be explained. This oversight (?) is not fatal IF we have good efficiency for burst detection in the ring counters AND reliable time (TDC) information between the central and ring counters. Unfortunately, these conditions are not well met in the Yale set-up, as we shall see. We are not happy with this situation; after analyzing it, we propose a solution based on what we have learned.

2. A major problem in the current experiment is the low efficiency for neutron-burst detection coupled with the short running time (103 hours of foreground data, 28 hours of background). Al demonstrates in his report that the efficiency of the set-up for detecting neutron bursts is much less than reported in the Interim Report. The burst-detection efficiency seemed suspiciously high to me and the efficiency for detecting two neutrons in one ring counter suspiciously low. Al flagged the "100%" efficiency statements in the Interim Report:

> <u>The most probable</u> neutron multiplicity (M_n) can be calculated from an event of fold (K) using the efficiency for detecting an event of fold K given by:
>
> $$\epsilon(K) = M_n \times \epsilon(12) * (M_n-1) * \epsilon(11) * \ldots * (M_n-K) * \epsilon(12-K)$$
>
> For neutron source of multiplicity 125 we obtain 100% efficiency for detecting a neutron in each detector (fold-12), and for multiplicity of 20 we have 100% efficiency of detecting 5 neutrons. Hence a 5 (6) fold event most probably corresponds to a source with neutron multiplicity of M_n = 20 (24).

The equation used in the Interim Report (above) leads to efficiencies for neutron detection greater than unity - see next page.

Calculating the burst-detection efficiency can be done using Monte Carlo techniques to generate neutrons and to evaluate detection probabilities in the counters. Al took the time to write such a Monte Carlo and proceeded to calculate the efficiency of the Yale set-up as a function of the multiplicity (M) of the burst. He also provides an analytical expression which he used to check the Monte Carlo. The results are given in tables in his write up. Note that the probability for detecting a burst of 125 neutrons in <u>all</u> ring counters is less than 5%, rather than 100% as stated erroneously in the Interim Report. Similarly, the probability of detecting 20 neutrons in 5 counters (2 central and 3 ring) is only about 15% (~6% when reasonable electronic inefficiency is folded in)--not 100% as stated in the Interim Report.

On the other hand, the Monte Carlo calculation shows that the probability for detecting two neutrons in a ring counter for a large burst is not negligible (stated as 0.2% in IR). A related problem with low efficiency for burst-detection is that interpreting the initial multiplicity from the number of detectors fired is ill-defined, as shown in Al's tabulated probabilities. Thus, for example, the multiplicity 5 event in run 45 in the IR, could have originated from a burst of 50-100 neutrons, and cannot be rejected as a burst candidate just because counter U5 shows greater than 2.5 MeV energy. A burst this large also has a significant chance (~50% for 100 neutrons) of self-veto since there was no shielding placed between the veto counters and the sample cylinders. In particular, the Monte Carlo calculation shows that a burst of 50 neutrons has ~25% probability of producing the counter-hit pattern of "event 45" including a double hit in U5. (See Table 1.) This is not negligible. It is worth noting that this event occurs at 90 minutes into warm-up from LN_2 temperature, corresponding to a sample temperature of about -10°C. On the other hand, event 45 could easily represent a cosmic-ray spallation event of, say 20 neutrons. We conclude that the low probabilities for burst detection coupled with low statistics (and other problems discussed by Al) preclude unambiguous evaluations of such events. With more data on time-structure of bursts, one might be able to distinguish cosmic-ray events from "real" bursts. As we discussed months ago, cosmic-ray induced particles might appear within nanoseconds whereas any "real" events could well produce neutrons spread out in time, perhaps over microseconds. (See Table 1.) (Problems with timing information are discussed in parts 1 and 3.)

One might be able to compensate somewhat for the low burst-detection efficiency of the Yale set-up with ample statistics from very long running, but we certainly don't have it. Samples used at Yale were taken to Brookhaven for longer examination as we agreed; I will leave it to Kelvin to discuss results. We must not ignore these results. However, the BNL detector cannot give us the information we seek regarding the initial time distribution of neutrons in a burst.

Let me give an example which I began with Moshe which shows immediately a neutron-detection efficiency(ϵ) > unity, using Moshe's formula.

Consider a neutron multiplicity of 200: what is $\epsilon(k)$ for $K = 10$ ring counters hit, each having $\epsilon_i = 0.008$ according to the Interim Report?

		(multiplying)
Then $\epsilon(10) =$	$200 \times (0.08) \times$	16
	$199 \times (0.072)$	14.3
	$198 \times (0.064)$	12.7
	$197 \times (0.056)$	11.0
	$196 \times (0.048)$	9.4
	$195 \times (0.040)$	7.8
	$194 \times (0.032)$	6.2
	$193 \times (0.024)$	4.6
	$192 \times (0.016)$	3.07
	$191 \times (0.008)$	1.52

$\epsilon \cong 3 \times 10^8$!

So $\epsilon(10) \gg 1$!

This immediately raises a flag, which lead Al to his revised formula and Monte Carlo calculations which agree and are sensible (e.g., no probabilities >1).

I'm afraid that the design of the Yale experiment and the data interpretation in the Interim Report are to a large extent based on a faulty formula.

[illegible signature]

If our analysis is correct, then the burst-detection efficiency given in the Interim Report is greatly overoptimistic and the conclusions that follow from it are misleading. In particular, the stated upper limits on neutron bursts are way too low. Let's look once again at the number of bursts of 20 or more neutrons expected in the Yale set-up, based on the Menlove results and efficiency numbers from Al's report:

Number of bursts expected at Yale:

$$ {}^{N}\text{Yale} = {}^{N}\text{LANL} \times \frac{{}^{T}\text{Yale} \times {}^{M}\text{Yale}}{{}^{T}\text{LANL} \times {}^{M}\text{LANL}} \times \frac{{}^{E}\text{Yale}}{{}^{E}\text{LANL}} $$

$$ = 39 \times \frac{103 \text{ hrs} \times \sim 200\text{g each hr}}{13{,}000 \text{ hrs} \times \sim 100\text{g each hr}} \times \frac{{}^{E}\text{Yale}}{{}^{E}\text{LANL}} $$

$$ = 0.6 \text{ bursts } ({}^{E}\text{Yale}/{}^{E}\text{LANL}) $$

We multiply the time (T) at each lab by the mass (M) of material per measurement hour, (~200g at Yale, ~100g at LANL). The number of bursts detected (^{N}LANL) is given in the Menlove Update sent out the collaboration and is 39 bursts of ≥ 20 neutrons over 13,000 measurement hours (at ~100g/hr). One may quibble over corrections to this calculation---for instance, using time spent in the -80° to 0°C range instead of total foreground running time, and the number of bursts detected at LANL in this range, increases the expected yield by about 3.

But the overriding factor is the ratio of efficiencies (E) for burst detection (^{E}Yale/^{E}LANL). Our analysis shows that the Interim Report was misleading on this crucial point. In particular, ^{E}Yale drops off sharply at N < 100. For example, the efficiency for detecting 20 neutrons is ~100% in the Menlove counter, while only about 12% in the Yale set-up if we require 3 or more ring counters to be hit (not 100% efficient as stated in the Interim Report). This drops to about 6% when reasonable inefficiencies in the electronics are folded in (see Al's report). Thus, the ratio of burst-detection efficiencies for ≥ 20 neutrons in a burst is significantly less than 1, so that:

^{N}Yale < 1 burst ≥ 20 neutrons expected, primarily because of the low efficiency for burst detection. *

3. In addition to the problem of low efficiency for neutron-burst detection in the Yale set-up, other difficulties should be reported because they show what can be improved in future experiments. Figs. 1 and 2 show what appear to be electronic artifacts/ saturation in detectors. While not necessarily fatal, these artifacts should be cleaned up or at least understood. Fig. 3 shows the existence of considerable cross-talk between timing channels. It is evident that a stop in one counter may generate stops in other timing channels, which may invalidate the time information in these other units. Now, imposing latches cleans up

* add factor for 20μs time gate on bursts at Yale versus 128μs " " " " at LANL (Menlove), so bursts expected << 1 at Yale - SEJ.

the problem appreciably as shown in Fig. 3b. But this imposes the constraint that neutrons be DETECTED within a 500 ns window and reduces what we can say about bursts -- we don't know the time structure of bursts in advance and should not bias the observations by imposing a short window on detection. I was assured in August that we could analyze the data without imposing the latch constraint, but this looks impossible with regard to timing information. Furthermore, there is a significant probability of multiple hits in counters for a burst event and there is ambiguity in the timing information between neutrons emanating from the target versus neutrons recoiling from other detectors.

What can be done? Certainly the problems in the set-up and the Interim Report outlined here and in Anderson's report cannot be ignored and the IR submitted for publication with minor changes. We need to take what we have learned and fulfill our commitment to do a careful, definitive experiment.

I therefore propose an improved follow-up experiment. I am willing to run again at Wright Laboratory if the improvements discussed herein are made and if sufficient running time is available. Otherwise, I propose an experiment using the muon-catalysis neutron detector at LAMPF. I have been a spokesman for muon-catalyzed fusion experiments at LAMPF for nearly eight years; we may use the system until May 1990 when the 800 MeV proton beam recommences. The LAMPF apparatus has been used successfully to detect 150 neutrons (average) produced over several microseconds by muon-catalyzed fusion and can be reconfigured for cold fusion. The electronics are already in place and can be used with minor modifications. Quantities of shielding materials are also available while the proton beam is off (until May). The time-structure of neutron events from muon catalysis has proven extremely informative and we have the tools needed to evaluate the time spectra (Phys. Rev. Lett. 56, 588 (1986), PRL 51, 1757 (1983), Nature 321, 127 (1986), etc.).

The apparatus uses 5 liquid-organic scintillators in 5" diam. x 5" deep cylinders. We would probably configure 4 in a tetrahedral arrangement to get the best burst-detection efficiency with the fifth detector to detect backgrounds simultaneously. Of course, we would perform Monte-Carlo calculations (as we have done for our muon-catalysis work) before settling on the configuration of detectors/samples. The Monte Carlo would include effects of neutron recoil in the counters and geometrical effects stemming from the fact that the sample is extended rather than point-like. (Note that such a study has not been done for the Yale set-up.) A brief write-up on the electronics by A. J. Caffrey is enclosed to provide further information.

Here are improvements expected relative to the experiment at Yale:

1 -- Good efficiency for detecting bursts of neutrons; the four or

five central counters will be timed together (this is already in the LAMPF electronics). We will use a Monte Carlo to assess whether shielding is needed between counters (or higher energy thresholds) to permit differentiation of neutrons coming from the target versus neutrons recoiling from other counters. Shielding will be placed between the cosmic-ray veto and the sample cylinder to reduce the probability that burst neutrons (if any) will generate a false veto.

2 -- No cross-talk between TDC's. Gains between the 4 detectors are rather closely matched at LAMPF facilitating determinations of the efficiencies of the counters when various n/g cuts are later imposed. The n/g discrimination at low-energies at Yale is good, but we do well at LAMPF (see attached report by A. J. Caffrey) and the system is well understood and clean after seven years of use. The logic-router circuitry at LAMPF (see Caffrey report) improves handling of multiple hits in a single counter. In particular, we can handle up to eight neutrons per detector, using 8 separate ADC's and 8 TDC's, so that we circumvent the ~80 μsec deadtime in ADC's at Yale. ADC's for pulse-height information are charge-integrating rather than peak-sensing. An electronics diagram will be provided.

3 -- We have approval to use the equipment until the end of April (the proton beam restarts in May 1990). We should plan at least 2 weeks for the first set of experiments -- say in Feb. 1990. We will want first to characterize the background using our on-line analysis package. Following data analysis, we can run again (in April) if necessary. Thus, we will have a longer time than at Yale in August and opportunity for follow-up experiments. The beam is scheduled through Sept., when we can run again if necessary.

No, it won't be easy or "quick and dirty." But by pooling our resources and building on what we have learned, we can together do the definitive experiment we set out to do.

P.S. In addition to our current collaboration, I have invited Menlove -- who was invited before but could not come -- and experts on the LAMPF system (Anderson, Caffrey and Paciotti) who have all said they would be glad to participate should we decide to run at LAMPF.

P.P.S. We may also be able to do experiments with deuterium plus TRITIUM. The needed safety equipment is in place at LAMPF from muon catalysis experiments. We have had years of experience with d-t fusion experiments and will need the help of Richard Maltrud of LANL for experiments involving tritium.

Sincerely yours,

Steven E. Jones

Steven E. Jones
Brigham Young University

Table ~~III~~ I Summary of High Fold Events (from Interim Report)

Detector	Energy Deposited (MeV)	Time of Flight (nsec)	Pulse Shape
Five-fold event in background Run 40:			
U0	2.8	0.0	n
U2	0.9	3.7	n
U4	0.9	3.7	n or γ
U5	4.4	-2.4	n or γ
D2	1.5	N/A	n
Five-fold event in background Run 51:			
U3	0.4	N/A	n or γ
U4	2.0	N/A	n
U5	4.4	N/A	n or γ
D0	0.9	0.0	n or γ
D1	1.5	3.7	n or γ
Five-fold event in data Run 45: Temp ≈ −10°C			
U0	1.8	0.0	n
U1	1.1	-17	n
U3	0.6 multiple hit possible	3800	n
U5	4.4	-18	n
D0	3.3	N/A	n
Six-fold event in data Run 61:			
U1	0.6	N/A	n
U4	0.4	N/A	n
D0	5.4	N/A	n or γ
D2	1.6	740	n or γ
D4	1.1	1.2	n or γ
D5	4.2	1000	n

Ring counter could be hit before central counter; reliability of times suspect

Solid neutron identification
Could be "real" or cosmic-induced – see text

Both central counters fired (but no relative timing in set-up)

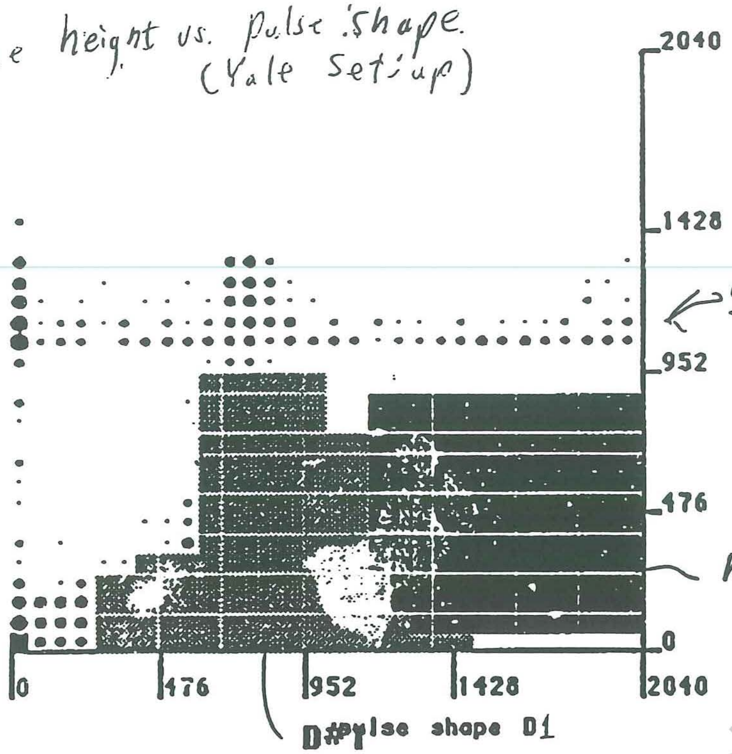
loss of timing information since U0/D0 not timed together

This drops out in Al's 2-dimensional (pulse-height + pulse-shape) cut due to high energy

(+ better timing)

Need more data to assess whether background bursts particles are typically separated by nanoseconds, and "real" foreground bursts by longer time intervals

Fig. 1. Pulse height vs. pulse shape (Yale Setup)



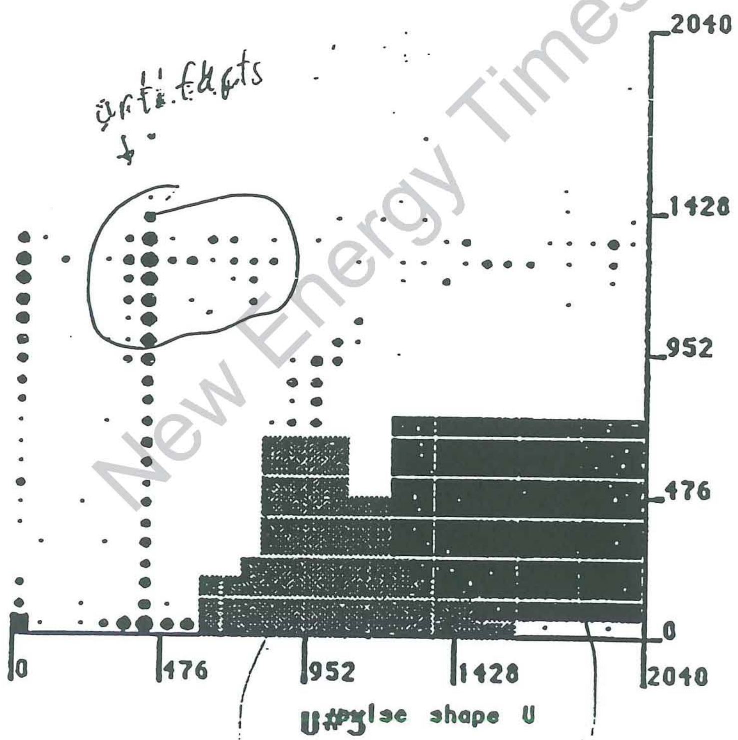
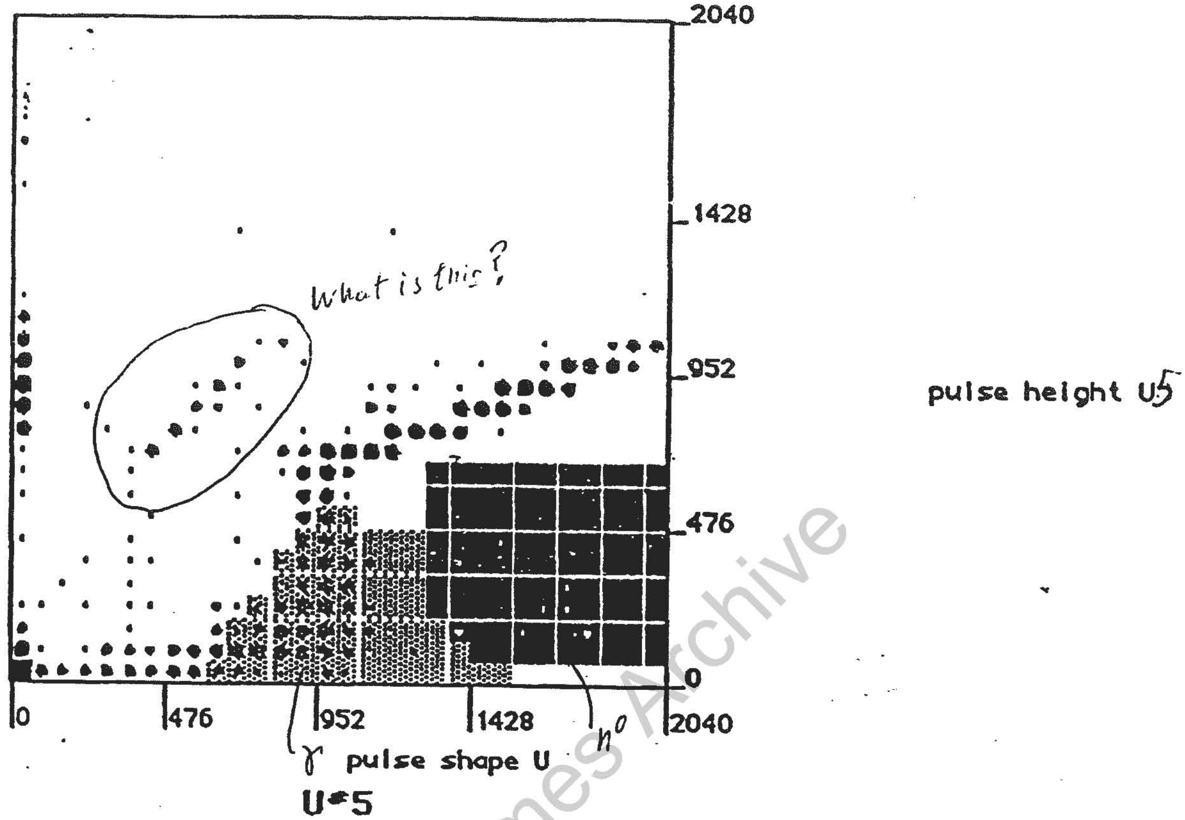


Fig. 2 Pulse height vs. pulse shape (Yale set-up)



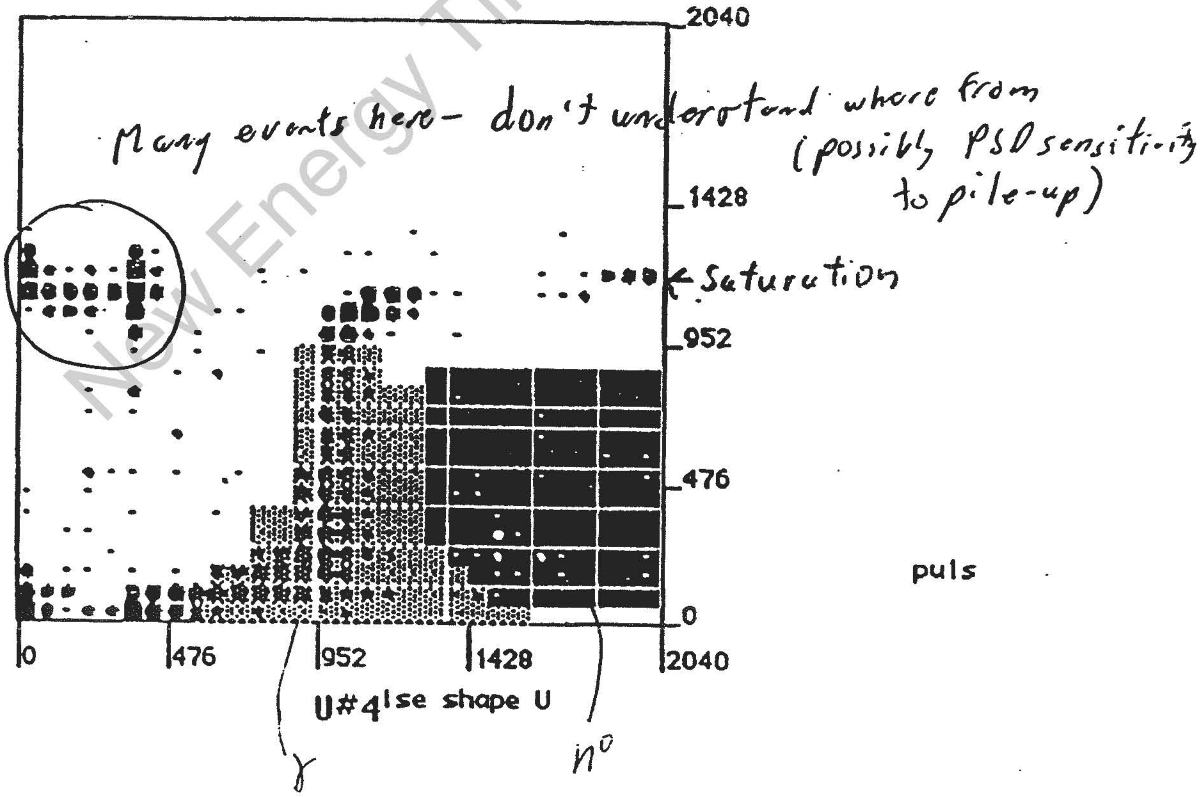
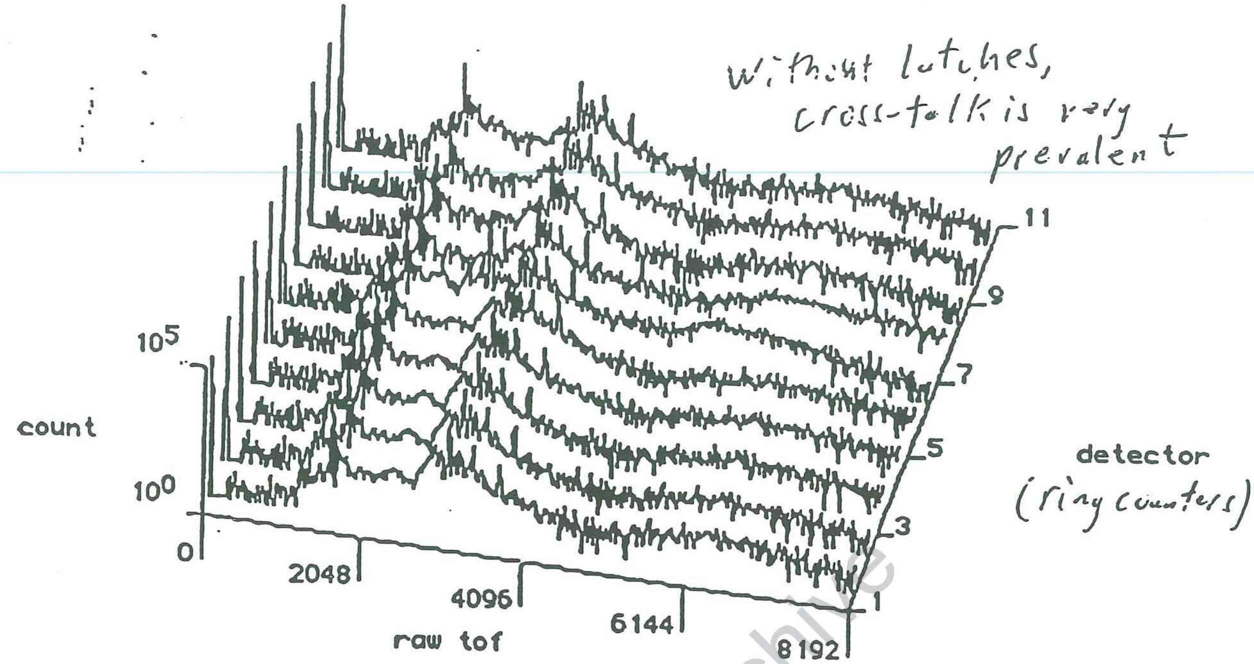


Fig. 3a. Timing Information/Cross-talk
(Yale set-up)

without latches, cross-talk is very prevalent

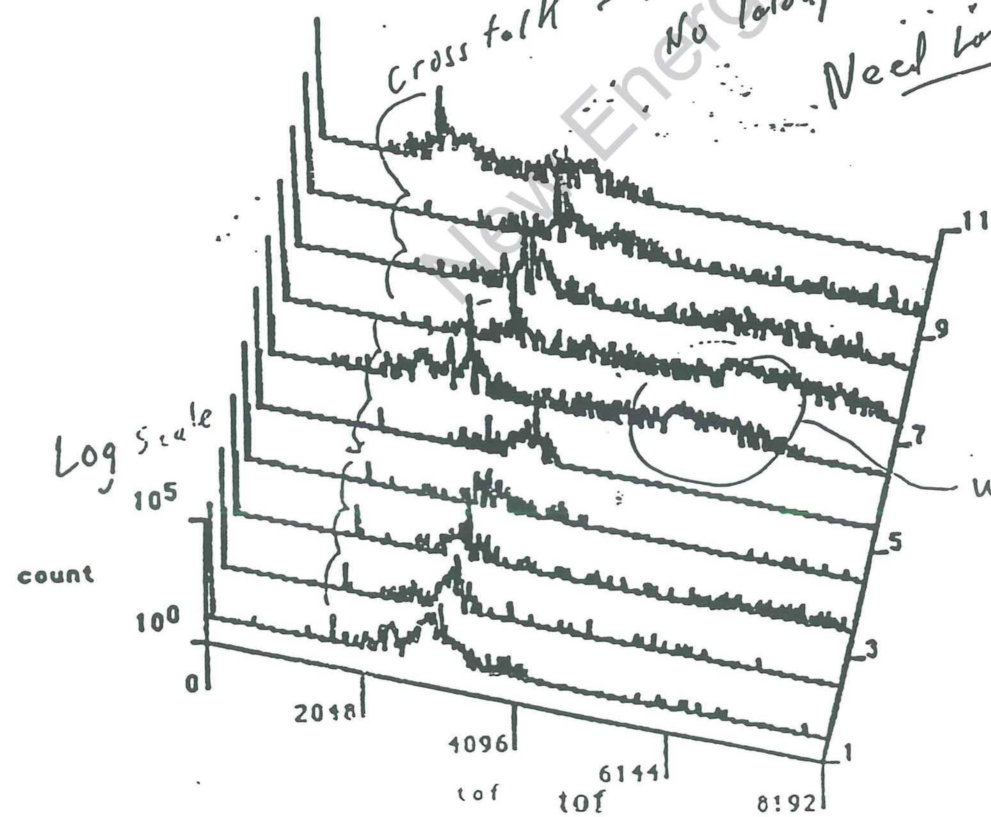


Raw TOF
40BG

Figure 3b. Timing information

cross talk – with latches required.
No latch, cross-talk much worse.
Need Latches, ∴ 500ns window imposed.

Raw TAC
Run 42



what is this?

detector (ring counters)

cross-talk (note log scale)

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CC:
Subj: Jones data analysis

Received: from YVAX.BYU.EDU by VANLAB.BYU.EDU; Mon, 8 Jan 90 16:24 MST
Received: from JNET-DAEMON by YVAX.BYU.EDU; Mon, 8 Jan 90 16:22 MST
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Received: by YALEVM (Mailer R2.03B) id 3171; Mon, 08 Jan 90 18:20:04 EST
Date: Mon, 08 Jan 90 16:48:36 EST
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Resent-date: Mon, 8 Jan 90 16:23 MST
Resent-to: JONESSE@VANLAB.BYU.EDU
Resent-message-id: <F843EF89B69F2002E7@YVAX.BYU.EDU>
Message-id: <F843FB5A015F2000BA@YVAX.BYU.EDU>
X-Envelope-to: JONESSE@VANLAB.BYU.EDU

cc: R. Gajewski

(A copy to Gajewski, as I recently learned from him that he has been getting copies of Jones communincations to me in the past).

Dear Steve,

Thanks for sending to me a copy of your analysis of the Yale data. Unfortunately, this package does not represent an analysis of the Yale data, since I don't see any plots of bursts detections, random emission etc. per run. Instead this package represent, as I shall detail below a false use of the Yale setup. I only wish you or your student would have called me to discuss it before you waisted such a long time.

In addition unfortunately, the report prepared by your student is infested with misundersatnding of what was done, as I shall detail below. Again I wish he would have returned my calls and discussed with me his work, it would have saved a great deal of time for him and all of us. I understand that you are eager to show that the Yale experiment is flawed, but you should also realize that understanding the set up is crucial for analyzing the data.

I shall reply point by point to your questions:

1. You suggest that there is a flaw in the setup, since the time between the two central detectors is not measured. Let me then remind you that in order to study burst emission over 20 microseconds with a few nanosecods accuracy (as we set out to do) we were obliged to use separate TACs in conjunction with ADCs for each detector. No existing TDC even at the high energy electronics pool in Brookhaven would have allowed us to perform this task. Since we have in our laboratory only 6 TACs, a request was sent to all laboratories around us to provide us with the necessary electronics. You will recall that we could only get extra electronics from Stony Brook, Brookhaven and MIT-Bated linear accelerator. You promissed to look into bringing more TACs, but found none. Since we could come up with only 12 TACs a decession was maid to have two separate hemispheres each working independently and to require that the timing of the two central detectors are matched. bad decision! Why was I not informed of this until October?

Since the timing of the two central detectors are matched, and since they both have to fall within a gate of 20 microsecond (produced by either one of the central detectors), no neutrons from bursts are lost, over 20 microseconds!

What is then the flaw that you refer to? – high γ rate ⇒ deadtime! Throws away ~50%!

3. The cross talk as I metioned to you is a well known problem of the Phillips Scientific discriminators. A similar effect is seen in many experiments at the Brookhaven AGS (e.g. E814). The problem is well familiar and standard solutions exist. Unfortunately you chosed the worst solution, i.e. to gate on the latch. A better solution is to gate on information included in an ADC channel. For example if you have a completely open gate on the energy, i.e. all energies accepted but you require a Pulse height information of any kind then no cross talk exist, no data is lost and no stringent timing are imposed as in your solution which uses the latch.
cross-talk still; see Al's response and report.

You also mentioned some problem with saturation. Again you should realize that in order to have as low threshold (of 50-70 keV electron equivalen) as we used in our experiment (in fact the lowest ever used on a neutron detector of that kind in an active research!), this requires driving the gain very high, as we did, which required saturating the channel at some 4.5 MeV or so. What is the problem with saturation at 4.5 MeV, if one looks at 2.5 MeV neutrons?

2. Unfortunately your student used the wrong equation for hit probability. I am surprised you allowed his report to leave BYU in the form it did. So here are the comments on your student work:

A. The equation your student used to calculate hit probability is wrong. Consider the following situation with one detector of 100% efficiency: for a neutron source of multilplicity 1 his formula gives hit probability of 100%, correctly. For multiplicity of 2 it yield a hit probability of 0%. This happens since he requires one and only one hit and in the second case we have a 100% chance of a double hit. If you inspect in a similar way his formula for a detector with 50% efficiency and again only one edetctor and multiplicity ranging from 1 to N, you again will notice that in fact as the mutiplicity rises the efficiency decraeses, as one may expect for requiring one and only one hit in a detector. This is certainly not the case in our experiment since we have no idea whether in fact the hits are single or multiple. In fact for the central detectors we are for sure seeing doubles etc. even with multiplicity of 20. Your student should read the paper that I mentioned to you over Bitnet last Friday and carefully correct this mistake. Correction of this mistake would be done by summing over all single and all multiple hits. Another problem in your student analysis is that he assumes (may be without even knowing) that all detectors have the same efficiency. This is why in our report we don't give the formula with powers of epsilon but as e(i)s as the efficiency is i dependent (if all e(i)=e one retreave the exponent used by your student). The math for this is somewhat involved but already published, for example in the paper I e-mailed to you.

In the example we have above with one detector with efficiency of 100% and a neutron source of multilicity 2, using my equation yield an efficiency of 200%. In this sense it is simply interpreted as 100% probability for a double hit. In fact you will notice that the formula we wrote in our report corresponds to the first binomial times K!*e**K, and the rest of the factors in (1) need to be rewritten and summed over. While having efficiency larger than 100% as suggested in my formula are indeed strange, they simply have to be explained (i.e. they are not wrong). !

See Al's response

May I suggest that we discuss it so as to avoid all this waist of time on trivia.

In the report your student talk about some advertisement of veto efficiency. The veto efficiency was not advertized, it was measured! if your student need to understand this measurement I shall be happy to explain it, as detailed in the run book. If he is missing an electronic diagram he should get in touch with me and I will be happy to provide him with one. It is not clear at all from his report where was his analysis hampered by not having the electronics diagram. Could you clarify this statement?

Again I am very eager to bring our data to publication and I am not sure why it took some two monts to get to the point where we discuss how to calculate the efficiency of our detectcr? I am very interested in releaving all problems of communications, I am surprised that my calls to your students where never returned and that the data analysis is dragging so slowly, even though we are so far only discussing trivia.

Best Regards Moshe gai.

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To: JONESSE@VANLAB.BYU.EDU
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Subj: Correcting mistake of Jone's student

Received: from YVAX.BYU.EDU by VANLAB.BYU.EDU; Tue, 9 Jan 90 20:46 MST
Received: from JNET-DAEMON by YVAX.BYU.EDU; Tue, 9 Jan 90 20:45 MST
Received: From BYUVM(YMAIL) by BYUVAX with Jnet id 7326 for JONESSE@BYUVAX; Tue, 9 Jan 90 20:45 MST
Received: from YaleVM.YCC.Yale.Edu by VM.BYU.EDU (Mailer R2.05) with BSMTP id 6913; Tue, 09 Jan 90 20:45:09 MST
Received: by YALEVM (Mailer R2.03B) id 1280; Tue, 09 Jan 90 22:44:46 EST
Date: Tue, 09 Jan 90 21:44:53 EST
From: "MOSHE GAI, (203)432 5195, FAX:(203)432 3522" <GAI%YALEVM.BITNET@VM>
Subject: Correcting mistake of Jone's student
To: Steve Jones <jonesse@YVAX>, kelvin lynn <kgl%APSEDOFF.BITNET@VM>, Douglas Morrison <jeremy%UCBCMSA.BITNET@VM>, Peter Parker <pparker%YALEVM.BITNET@VM>
X-VMS-To: Steve Jones <jonesse@YVAX.BYU.EDU>, kelvin lynn <kgl@APSEDOFF.BITNET>, Douglas Morrison <jeremy@UCBCMSA.BITNET>, Peter Parker <pparker@YALEVM.BITNET>
Resent-date: Tue, 9 Jan 90 20:45 MST
Resent-to: JONESSE@VANLAB.BYU.EDU
Resent-message-id: <F7561EF454BF2007D4@YVAX.BYU.EDU>
Message-id: <F7562758C4BF2000BA@YVAX.BYU.EDU>
X-Envelope-to: JONESSE@VANLAB.BYU.EDU

Dear Steve,

I have been trying very hard to get in touch with you to discuss the trivial mistakes that your student (Al Anderson) makes in his attempt to analyze the Yale data. Steve the reprot you mailed to us is so bad that Pauli would have said: "it is not even wrong!". You could have at least asked the student to use a more polite language! To hear from him that the Yale experiment was performed by amatures, after all what this laboratory did to allow this research is just not acceptable. I understand that you are now circulating this false criticism to whoever would listen to you. We learned about it from Kevin Wolf at Texas A&M from Gajewski at DOE etc. In fact Kevin Wolf knew about it before myself. Let me assure you that Yale University will not take part in this discussion!

I have not discussed the analysis with Wolf – have not even spoken to him since mid-October (rec'd Yale tapes end Oct.)

Concerning the data analysis by your student. I feel that it is so bad that we might as well just ignore it. I will pretend that I never got it, in order to save you some embarrasment. However I would be happy to discuss it with you on the phone if you so desire.

Since your student managed to confuse you on the issue of calculating hit probabilities, I would like to take the time to correct the algebra. I send to you by FAX two pages of algebra. You can see that once one corrects the wrong formula used by your student and with a little bit of algebra one gets excatly the formula of our report times a factor: (M-K)/K! This factor is of the order unity in our case (M=12 and K=3) so it was neglected in the report. We can add it in the final publication.

incorrect; see Al's write-up

we need to handle M=20 to M=300 to check Menlove results, anyway

For calculating hit probabilities you need to know:

1. There are B(M,K) possibilities to chose K elements from a group of M, where B(M,K) is the binomial coefficient.

2. It is possible to have K! permutation of K elements.

3. The escape probability of N elements from D detectors with efficiency e each, is given by: (1-De)**N.

So the probability of getting K hits in an array of D detectors all with efficiency e, when we allow any order of detection, is given by:

B(M,K)*K!*B(D,K)*e**K*SUM [one escapes, two escapes, three escapes etc.] Incorrect - Al's response

You will notice that the sum stands for single hit (i.e. M-K escape detection) double hit etc. etc. While we know that the probability of double hits in each one detector is very small, when summing over all the combinatorical combinations a large number arise. So when you do this algebra, as I FAX to you, you will recover our formula in the DOE report.

Steve in our last phone conversation you promissed to me that once we solve the question of calculating the efficiency of our neutron detector, you will agree to publishing our results. Do you agree now?

Best Regards Moshe Gai.

Correctness and carefulness are far more important than hasty publication
- S E Jones

Response to Comments by Dr. Gai to My Analysis
of the Yale Cold-Fusion Experiment

A.N. Anderson PhD
9 Jan, 1990

Background

This is a response to comments by Dr. M. Gai of Yale University, on a report I did as a consultant for Dr. S. E. Jones, a collaborator with Dr. Gai, in an experiment which, in view of the impact it may have on future research, he felt should be carefully scrutinized to see what conclusions I felt were justified.

My report was to be only for the internal use of the authors, which they could use or not as they saw fit, and in that light I tried to be constructive. The questions I raised, I feel deserve real answers.

I will address the comments in the order they appear in the communication from Dr. Gai:

- Credentials: Dr. Gai assumes the analysis was done by a student of Dr. Jones, and as such is perhaps not to be taken seriously. Whether that opinion of my report is justified or not, my credentials are not irrelevant and I present them here. I received a PhD in Nuclear Physics from the University of Idaho in 1975. I have taken part in and analyzed experiments at LAMPF (Thesis on Proton-Proton Elastic Scattering, muon-catalyzed fusion), TRIUMF (University of Alberta, Proton-Proton Bremsstrahlung, neutron scattering, and muon-catalyzed fusion experiments) and Rutherford Appleton Lab, U.K., (muon-catalyzed fusion).
  Specifically I have been analyzing neutron-burst data from muon-catalyzed fusion experiments at LAMPF since 1983, in collaboration with Dr. Jones. The problems associated with these experiments are very similar to those encountered in the Yale experiment, and I can say with some assurance that I am as qualified as anyone to comment on them.
- Was this an analysis? My report included, among other items, a plot of burst detections, and two tables of neutron singles counts. I was able to infer both neutron singles and burst rates (within efficiencies). What I was *unable* to do was develop a satisfactory feel for the actual efficiency of the system. I raised questions which I think should be answered before conclusions are drawn.
- Flaws and TDC's: It is not my function to discuss *why* flaws occurred in the experiment, but rather *whether* they occurred. Time of flight is an extremely important criterion by which background gammas, etc. can be rejected. The way the experiment was set up, however could not allow time of flight from more than one neutron per central detector, and in the requirement of the latch (of which more below) means that time of flight was unavailable for events occurring in the second center detector over 500 ns. after an event in the first detector. I belabor this point because a little reflection would have shown that high resolution was necessary only for gamma rejection over a very short interval, and was certainly not needed over the entire 20 μs. Dr. A.

J. Caffrey solved a similar problem for our experiment at LAMPF in 1983, using logic routers and groups of TDC's of two different resolutions, which were readily available.

- Crosstalk: It is irrelevant whether crosstalk is common (In fact, I have never observed TDC crosstalk at all, let alone this magnitude, even in the 50 TDC's we have used for the last 7 years at LAMPF). The proper question is, does it cause problems? Figure 2 in my report shows a small number of events in detector D5 (run 52, Background) which satisfy all criteria I could put on them except time of flight. (i.e. Both latches, Central=neutron, Ring=gamma). Besides those with good times of flight, there are several events whose times of flight look suspiciously like the gamma peaks from other detectors. If the peaks of different detectors had been lined up (as several others were) there would be *no* way to distinguish them from good events. It is the burden of the authors to demonstrate that these events do not compromise the data.
- Latches: Dr. Gai implies that not only is it not necessary to look at latches, but that requiring latches throws away events! My plots of event gates show that almost all otherwise good events also had latches. A very large percentage of events which did not have latches had problems (like leftover TDC's and crosstalk). Requiring good ADC pulses does not allow the deadtime of the ADC to be evaluated. Likewise for the pulse shapes. The cause of bad pulses is likely to be pileup and as such reflects a decrease in efficiency. The effects I observed were as great as 70% in some detectors. The authors have a responsibility to show whether this effect is real and if so, has been accounted for.
- Saturation: As I stated in my report, amplifier saturation was not in itself a fatal problem. I was struck more by the lack of uniformity of all aspects of the pulses, including saturation, gain, thresholds and pedestals (Fig. 5, my report). Again, if I thought that these variations occurred *in spite* of the extreme care that had been exercised, I would not consider them important. It is the suspicion of sloppiness that worries me. When an experiment is acknowledged to be pushing the limits of experience, it is all the more necessary to take extreme care to ensure that these particular parameters, are carefully set and monitored. It is puzzling why *some* of the counters would have their pedestals *carefully* set to or *below* zero. Again, it is the burden of authorship to answer these questions to the satisfaction of readers.
- Probabilities: This is a most exasperating point. When I first saw the expression used in the 'Interim Report', I assumed it was the work of a naive student. I am astounded to see the same absurd expression being defended by a professor of Physics at Yale. Let us quickly dispose of the arguments against my expression. The particular expression I used is indeed the probability for one and only one hit in a detector. There were two reasons I chose it. First, it was as applicable to the experiment as any other assumption about multiplicities. (The argument has been made that events with *any* multiple hits might need to be rejected. In fact, that was the stated reason for rejecting the single 5-fold hit mentioned in the 'Interim Report'. See my report for discussion.) Second, it was easy to derive, and evaluate, and so was a good check on the MonteCarlo, which was my primary source of probabilities.

  I examined the source from which Dr. Gai claims he derived his expression, namely L. Westerberg *et al* <u>γ-Ray Multiplicities From Nuclear Reactions: (etc.)</u>, NIM <u>145</u>

(1977) 295-310. I was unable to find anything which remotely resembled the expression in the 'Interim Report'. The closest I could find to a relevant expression was the following (with variables changed to try to correspond with my expression):

$$P_{Dk} = (-1)^k \binom{D}{k} \sum_{i=0}^{k} (-1)^k \binom{k}{i} [1-(D-i)\bar{\epsilon}]^M \prod_{\beta} [1-(D-i)\epsilon_\beta]$$

Let met point out explicitly, that this expression is intended to be the probability of one *or more* hits in k detectors. It in *no way* contradicts the expression in my report! The expression looks plausible to me, it satisfies the more obvious requirements of a probability, but having derived several such expressions over the years, I have learned not to trust such complicated expressions without testing. I am not sure about the relevance of this expression to the 'Internal Report' and will leave testing of it to someone else. Let me dispose of Dr. Gai's arguments instead.

Consider Dr. Gai's examples. For 100% efficiency and multiplicity 2, the probability of detecting one and only one neutron is indeed zero, and an expression which gave any other answer should be rejected. Again, for 50% efficiency, one would expect my expression to give decreasing probabilities for large bursts, and again one would be right. This is exactly what one would observe if one rejected events with more than one hit. This is what will happen if one requires pulse information from central counters during large bursts. Let us now examine my expression for one detector of 50% efficiency and a burst of 2 neutrons. Simple probability tells us that the probabilities for 0, 1 and 2 hits are respectively, 0.25, 0.5, and 0.25. My expression yeilds 0.25 and 0.5 for the first two, and is not defined for the third, since it would be the probability of having 2 detectors get one and only one hit.

Dr. Gai claims that the 200% his expression yields can be interpreted as a 100% probability for a double hit. This is certainly not a probability or even an efficiency. It cant be an expectation for number of detectors hit, because it can easily exceed the number of detectors. Using this same reasoning, should the 100% one gets from 2 neutrons on a detector of 50% be interpreted as a 50% probability of a double hit? Perhaps a 33% probability of a triple hit?
Since this is not a correspondence course in elementary probability theory, I will move on. This discussion should not, however, be dismissed, because the differences in the rates it predict are so significant. It definitely is not a trivial point!

- Advertized Veto Efficiency: I use that word because, not having measured it myself, I was taking it as given. Perhaps the word 'reported' would be more appropriate.
- Analysis hampered by lack of diagrams: As I was eventually able to infer many of the properties of the electronics from the data themselves, the only thing that was lost was time. The authors seem to think that time is a fairly important commodity, however.
- Communication: I have spoken over the phone to Ralph Frances and found him helpful, but he admitted to some ignorance of the electronics. I left several messages for Steve Rugari, but he did not return my calls.

- Detector Efficiency: I reiterate, that in any experiment that attempts to measure neutron rates, detector efficiency is of utmost importance, and should be completely understood before publication. I am somewhat puzzled by the rush, however. Is it more important that the 'Interim Report' be published soon than that the results be correct?

Conclusion

I suggest that my report be taken seriously. Most of the points mentioned by Dr. Gai could have been cleared up by careful reading of the report. Several of my worries and questions may already have very satisfactory answers, and others may not significantly change the results, but they are all legitimate. The question of the probability distributions should be seriously addressed. I suggest consulting a recognized expert on probability. If this problem cannot be corrected, I would advise Dr. Jones and anyone else connected with the experiment to remove his name from the author list to avoid the real possibility of acute embarrassment.

To: Yale/BNL/BYU collaboration
From: S. E. Jones
Date: 11 January 1990

Dear Colleagues:

I find it an unpleasant task to respond to Moshe's comments on Alan Anderson's report, that "it is so bad that we might as well just ignore it," that "I don't see any plots of bursts detections, random emission etc. per run," that Alan "managed to confuse you on the issue of calculating hit probabilities," and so on. Al has responded, and I agree with him that a careful reading of our reports would have cleared up many of the questions raised by Moshe.

For example, random neutron emission is discussed on pages 1,2,3, 9 (relevant plot) and 15 of Al's report in some detail, including a table (p. 2) showing the per-run neutron singles rates and an appendix (p. 15) showing the actual singles counts in each ring detector per run. Burst detections are discussed in my report, and in Al's on pages 3,4,5,6,7 and related plots (Figures 3-7). I would like to call particular attention to Al's Fig. 4 which relates directly to the burst-candidate in run 45:

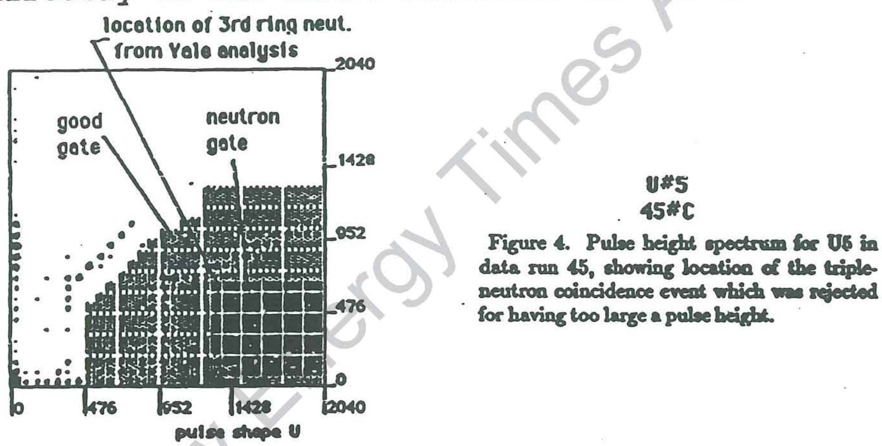


Figure 4. Pulse height spectrum for U5 in data run 45, showing location of the triple-neutron coincidence event which was rejected for having too large a pulse height.

Here Al shows the location of the pulse height (PH) and pulse shape (PS) for the detected hit in detector in detector U5 (see also Table I in my report and Table III in the DOE interim report). Al shows here that this point (PH=720, PS=1300) lies just above his 2-dimensional "neutron gate" and also near the bottom edge of the saturation band for this detector. As I argue in my report, this point could represent a double hit in detector U5, resulting in an energy higher than 2.5 MeV (about 4 MeV). But the saturation band interferes with an unambiguous interpretation, since the point could be associated with the saturation band -- a gamma, for instance. The saturation may thus reduce our ability to discern double hits; this responds to a couple of Moshe's questions. I suggest that the saturation band situation biases against high neutron multiplicities since these tend to have more multiple hits. The ability of the set-up generally to handle multiple hits is a problem I discuss also in my report.

Moshe excuses the lack of relative timing between the two sets of detectors (up and down) as due to lack of sufficient TAC's. But since these detectors are much more efficient than the ring counters, it would seem logical to sacrifice a ring counter's timing in the event of too few electronics modules in order to get timing information from the two counter sets. This oversight greatly impedes our ability to extract useful timing information from neutron bursts, as I discuss in my report. It is a major weakness in the set-up, in my opinion.

Moshe states that requiring pulse height information eliminates the cross-talk. However, Al demonstrates in Figure 2 of his report that evident cross-talk persists even with pulse-height information (and latches) required. (Al discusses this further in his response to Moshe.) I find that Al has done a good job of supporting his conclusions with tables and plots. Al has had about seven years of experience in looking at data coming from detectors such as those used at Yale and as such is in an excellent position to help us understand the data. We should be grateful that he was willing to help.

Now we come to what I argue in my report is a central issue: calculating hit probabilities. In his report, Al clearly states that the hit probability equation used is just for the "simple" case of having K detectors hit by 1 neutron only:

The proper expression, verified by MonteCarlo, for the probability $P_{M,D,\epsilon}(K)$ of having K detectors hit by exactly 1 neutron, and no other hits, given a burst of M neutrons and D detectors of efficiency ϵ, is

$$P_{M,D,\epsilon}(K) = \binom{M}{K}\binom{D}{K}K!\epsilon^K(1-D\epsilon)^{M-K} \tag{1}$$

where $\binom{M}{K}$ is the usual binomial coefficient. Other hit criteria or differing efficiencies can make the expression much more complicated. The report uses an expression which

To handle more complicated cases such as multiple hits (and detectors of differing efficiency, such as ring and central counters--see Al's report p. 3 bottom), Al uses a Monte Carlo calculation, an excellent approach. He then checks the Monte Carlo with the expression for consistency in the case of single counter hits (compare the last column [predicted probability based on the formula above] of his Appendix B. tables with the third column [relevant Monte Carlo calculation]). The agreement is good (remember, too, that just 10,000 throws were used in the Monte Carlo calculation per case).

Again, we rely on the Monte Carlo, not the formula, for multiple-hit cases. Al clearly states this in his report. But Moshe says the formula is wrong because it does not allow for multiple hits:

To: Steve Jones 1/8/90

From: Moshe Gai

Subject: Correcting your Student's (Al Anderson) Mistakes

He used the following <u>wrong</u> formula:

$$P(K) = \binom{M}{K}\binom{D}{K} K!\, \varepsilon^{K} (1 - D\varepsilon)^{M-K}$$

to correct it he needs to allow multiple hits in the detector, since our setup is blind to multiple hits. By summing over all combinations, while each multiple hit probability is small, a final large probability occurr.

so:

$$P(K) = \binom{M}{K}\binom{D}{K} K!\, \varepsilon^{K} \Big[\underset{\substack{\uparrow \\ \text{single hit}}}{(1-D\varepsilon)^{M-K}} + \underset{\substack{\uparrow \\ \text{double hit}}}{(1-D\varepsilon)^{M-K-1}} + \dots \Big]$$

Notice that Moshe's expression agrees with Al's for the case of single hits, which is, of course, all that Al's expression was used for. Moshe's expression for multiple hits, however, is questionable and there are definite errors in getting from that to the (incorrect) formula used in the Interim Report. Al has explained these problems in detail and I will urge you to carefully read his comments.

Faced with the same (non-trivial!) problems as described in our long reports, I draw the same conclusions on the best path to follow now in our quest to do a good experiment. I think we should take what we have learned and carefully design and execute an experiment to examine the time-structure of possible neutron burst events. I have recommended the use of the segmented neutron detector at LAMPF (Los Alamos) for the reasons given in my report. This detector has been successfully used in the past to analyze the time structure of bursts of neutrons arising from muon-catalyzed fusion and can be applied to the task at hand.

However, I have learned to my dismay that someone called the director of LAMPF (G. Garvey) last Tuesday (Jan. 9, 1990) and urged him NOT to allow us to do this particular experiment! My response is that there are other places where we can do the work, if necessary, including foreign countries where we have collaborative arrangements with muon-catalyzed fusion research groups. Scientific research can clearly be impeded, but it cannot (ultimately) be blocked.

Gentlemen, we intend to continue in this research and to do our best. I extend an invitation to all collaborators to come to BYU soon for a discussion of the problems and the best solutions.

Sincerely, and perhaps a bit more
firmly than usual for me,

Steven E. Jones

Steven E. Jones

cc: R. Gajewski, Douglas Morrison, Peter Parker (since Moshe has them in related communications, to which we are responding)

From: IN%"GAI%YALEVM.BITNET@VM" "MOSHE GAI, (203)432 5195, FAX:(203)432 3522"
To: JONESSE@VANLAB.BYU.EDU
CC:
Subj: Hit probability

Received: from YVAX.BYU.EDU by VANLAB.BYU.EDU; Thu, 11 Jan 90 15:33 MST
Received: from JNET-DAEMON by YVAX.BYU.EDU; Thu, 11 Jan 90 15:32 MST
Received: From BYUVM(YMAIL) by BYUVAX with Jnet id 6770 for JONESSE@BYUVAX; Thu, 11 Jan 90 15:32 MST
Received: from YaleVM.YCC.Yale.Edu by VM.BYU.EDU (Mailer R2.05) with BSMTP id 4354; Thu, 11 Jan 90 15:32:20 MST
Received: by YALEVM (Mailer R2.03B) id 8132; Thu, 11 Jan 90 17:32:27 EST
Date: Thu, 11 Jan 90 17:23:56 EST
From: "MOSHE GAI, (203)432 5195, FAX:(203)432 3522" <GAI%YALEVM.BITNET@VM>
Subject: Hit probability
To: Steve Jones <jonesse@YVAX>, kelvin lynn <kgl%APSEDOFF.BITNET@VM>, Douglas Morrison <jeremy%UCBCMSA.BITNET@VM>
X-VMS-To: Steve Jones <jonesse@YVAX.BYU.EDU>, kelvin lynn <kgl@APSEDOFF.BITNET>, Douglas Morrison <jeremy@UCBCMSA.BITNET>
Resent-date: Thu, 11 Jan 90 15:32 MST
Resent-to: JONESSE@VANLAB.BYU.EDU
Resent-message-id: <F5EF7662D3BF200EC6@YVAX.BYU.EDU>
Message-id: <F5EF8043355F2000BA@YVAX.BYU.EDU>
X-Envelope-to: JONESSE@VANLAB.BYU.EDU

Dear Steve,

It was good to talk to you on the phone. I am happy that you disagree with the style of writing of your assistant Al Anderson.

really?

The problem of calculating hit probability is very simple and exact solutions were given by B.R. Mottelson in 1974. In the interim report we wrote down first order approximation of the exact solution. It is O.K. for low multilicities as is the case here. no!

From my graph you can see that for a multiplicity of 20 we have 85% chance of observing fold 4 or 5 or 6 or 7 or 8 or 9 or 10. Since we do not observed in the ring counters more than a fold=3 event, than with 85% confidence we can say that no events of multiplicity larger than 20 were observed. NO!

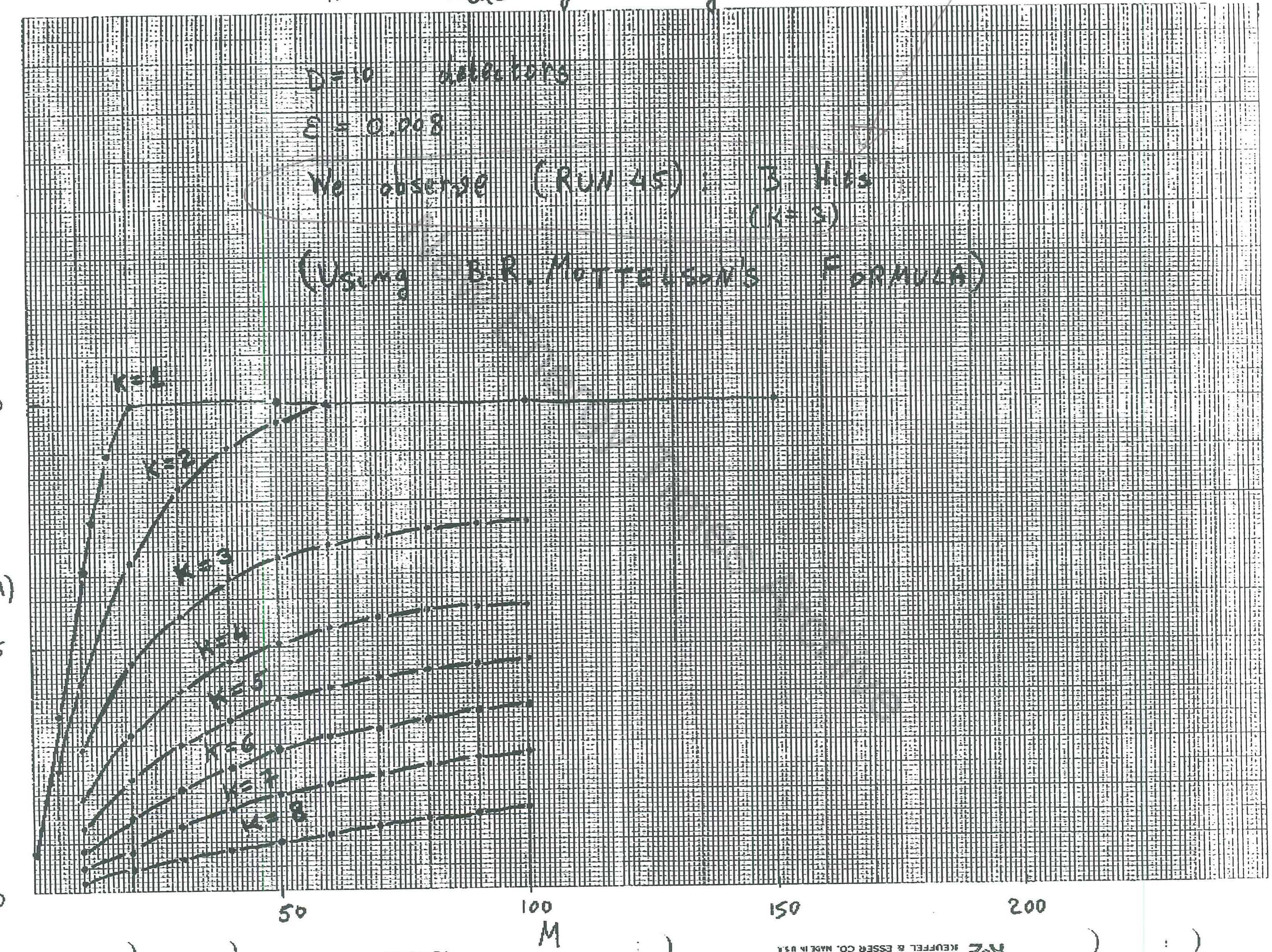
what?

Best Regards Moshe Gai.

Al's Monte Carlo for $P \geq 4$ ring counters and $M = 20$:
$P \cong 4.3\%$ not 85%!
-SEJ

Hit Probability in Ring Counters

NOTE (1/11/90 – Moshe)



To: Yale/BNL/BYU collaborators and interested parties
From: S. E. Jones
Date: 15 January 1990

Dear Colleagues,

As you know, we have been carrying on a dialog regarding the interpretation of the data acquired using the Yale apparatus last August. Enclosed find the reports by myself and Dr. Alan Anderson, Moshe's responses, and our replies to Moshe. I would be willing to continue this dialog, except for two watershed factors which I learned about just last week and which I discuss below. The decision to discontinue working with Moshe has been made after careful deliberation this past weekend, and out of respect for all concerned, I provide the reasons here. Please understand that the attached documents provide relevant information that I ask you to consider in addition to this brief letter.

I. The probability of detecting neutrons from a Menlove-type burst (approx. 20 to 300 neutrons) must be well-known in order to interpret the data, and indeed to assess the efficacy of the experimental set-up. We have been using the following formula supplied by Moshe:

(Interim Report, pg. 5)

> **<u>The most probable</u> neutron multiplicity (M_n) can be calculated from an event of fold (K) using the efficiency for detecting an event of fold K given by:**
>
> $$\epsilon(K) = M_n \times \epsilon(12) * (M_n - 1) * \epsilon(11) * \ldots * (M_n - K) * \epsilon(12-K)$$
>
> **For neutron source of multiplicity 125 we obtain 100% efficiency for detecting a neutron in each detector (fold=12), and for multiplicity of 20 we have 100% efficiency of detecting 5 neutrons. Hence a 5 (6) fold event most probably corresponds to a source with neutron multiplicity of M_n = 20 (24).**

Alan Anderson and I demonstrate that this formula is incorrect (attached reports). I concluded: "the design of the Yale experiment and the data interpretation in the Interim Report are to a large extent based on a faulty formula," and "the burst-detection efficiency given in the Interim Report is greatly overoptimistic and the conclusions that follow from it are misleading." (Jones, 1/3/90 report) Here is Moshe's response (see more attached):

$$P(K) = M\,\varepsilon(D) * (M-1)\,\varepsilon(D-1) * \ldots * (M-K+1)\,\varepsilon(D-K+1) * \frac{(M-K)}{K!}$$

You will recognize here <u>our formula</u> from our report, <u>multiplied</u> by a factor $\frac{M-K}{K!}$

This factor in our case ($M=12$, $K=3$) is of the order unity and was neglected in the report.

<u>THERE IS NO MISTAKE IN THE REPORT!</u>

Moshe, I felt that your stubborn defense of your formula, including justifying probabilities greater than unity, was obtuse, but still I thought we might reach an understanding. But then late last week (Friday) I received a copy of a letter to you from Richard Garwin, dated November 1, 1989. Dr. Garwin states that the neutron detection efficiency formula in the Interim Report is incorrect, and adds:

"I would like to receive from you a corrected version of the manuscript which does not have this misleading statement, and which does the analysis right." In your response, you agreed. These facts (which I learned late last week) lead me to raise strong objections.

Why was I not informed of Dr. Garwin's insight earlier? When Alan and I independently dug out the same conclusion, why your stubborn struggle to maintain the validity of the repudiated expression (see attachments)? Why argue that Alan Anderson "managed to confuse you [Jones] on the issue of calculating hit probabilities" in view of the insight from Dr. Garwin? Finally, how could you let the paper reside with the DoE/ERAB committee uncorrected, when you had learned from Garwin that the Interim Report was "misleading"? I note that your infamous press release which accompanied the release of the Interim Report to the media was at this same time that Garwin caught the error -- you should not then have gone to the press!

In view of these matters, I am not willing to continue the collaboration with Dr. Gai. But there is more.

2. Moshe will not concede the seriousness of electronics and other problems in the Yale set-up which are plainly visible in the data analysis (see esp. Anderson report). Anderson and I show that the saturation in pulse-height information in some counters and the cross-talk between timing channels (which does persist when ADC information is required, incidentally) and other electronics untidiness cause difficulties in the data analysis. The lack of timing between up and down detector sets is a major weakness of the set-up, in my view (attachments).

In my January 3 write-up, I noted these problems along with the low efficiency for burst detection of the Yale set-up. I proposed that we take what we had learned and do an improved follow-up experiment, either at Yale or at LAMPF. I then discussed the improvements which could be realized with the LAMPF neutron detector system, used in the past for muon-catalyzed fusion studies. I hoped that this approach would avoid embarrassment -- it is not unusual to have to re-do an experiment to correct problems.

But instead of accepting this solution or even discussing it with me, Dr. Gai called the director of LAMPF (G. Garvey) last week.* I am not privy to the details of that conversation, but I was informed that "pressures" could stop the experiment, and that the possible reaction of the physics community would have to be evaluated before the experiment could be pursued, as a direct result of the conversation. Mind you, the proposed experiment would use equipment which is dormant while the LAMPF beam is off (until May) and had received prior informal approval from Jim Bradbury. It would be conducted with very few demands on LAMPF personnel. Certainly, Moshe's phone call did nothing to help the follow-up experiment to which he had been invited, and is not indicative of a cooperative collaborator.

I have explained salient reasons for my decision to

* There is some question on whether Dr. Garvey returned Dr. Gai's call or called first ~~or~~ was called — no matter! I learned from James Bradbury of LAMPF of the impact and nature of Dr. Gai's conversation, discussed above — Jim had spoken to Dr. Garvey over lunch on 1/9/90 about this — SEJ.

discontinue collaborating with Moshe. Please accept my statement that I bear no personal animosity towards you, Moshe, although several of your remarks and actions in this collaboration have cut me deeply as I have told you before. I am saddened by these events, but hopeful that we can make use of what we have learned in the effort to better the research.

Sincerely,

Steven E. Jones

Steven E. Jones

Note added: I have some evidence also that Moshe has impeded publication of the Menlove paper in Nature, outside the normal peer-review process — SEJ.

1st time I have seen this! Heard about on 1/11 from Kelvin
- JEJ

Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598
(914) 945-2555
FAX: (914) 945-4419, Telex: 137456 IBMRESRCH YKHG UD,
BITNET: RLG2 at IBM.COM

November 1, 1989
(Via BITNET to GAI at YALEVM)

Professor Moshe Gai
A.W. Wright Nuclear
Structure Laboratory
Yale University
New Haven, CT 06511

Dear Moshe,

I have seen your "Upper Limits on Neutron Bursts ..." but before I try to follow the analysis in detail, I wanted to ask you about something that caught my eye. You say "For neutron source of multiplicity 125 we obtain 100% efficiency for detecting a neutron in each detector (fold=12), and for multiplicity of 20 we have 100% efficiency of detecting 5 neutrons." I understand that with a 0.8% detector efficiency and the multiplicity of 125, you have an expected number of neutrons counted in each counter of 1.000, but that by no means corresponds to "100% efficiency." In fact, it corresponds to (1-1/e) or 63% efficiency.

Of course you know this, but I would like to receive from you a corrected version of the manuscript which does not have this misleading statement, and which does the analysis right.

Thank you for providing this beautiful experiment. I hope that the further projected analysis is available soon.

Sincerely yours,

Richard L. Garwin
Forwarded in his absence

20 x 0.008 x 10 ring counters
= 1.6 in rings →
can't get ε = 100% in
5 counters, even w/ 2 in central
Garwin is sharp!
Al Anderson & I found
~some non-trivial
error, but it took
weeks, He is also
right that analysis in
DOE report is misleading.
wow!
Moshe's 1/8/90
memo says his
formula is
"not wrong"!
- JEJ

RLG:jah:305%MG:110189..MG
} RLG2 YKTVMV 11/01/89
~ R.L.Garwin GAI@YALEVM 11/01/89 Interpretation of expt with J
=== 21

Date: Wed, 01 Nov 89 16:51:57 EST
From: Moshe Gai <GAI@YALEVM>
Subject: Re: Interpretation of expt with Jones, et al.
To: RLG2@WATSON
In-Reply-To: Message of 1 November 1989, 16:19:20 EST from <RLG2@YKTVMV>

Deaf Dr. Garwin,

Yes I agree. Please note that the manuscript was written in a day. On October 3rd I suggested that we submitt a report but Jones refused to submit a report to DOE/ERAB panel. I did not plan to submit one due to his objection. We only received Jones OK for submitting a report on the data which was analyzed over a month ago, by October 24th. and a report had to be written, Figures made, all reach an agreement and report be submitted by Oct. 27th! While the report includes data that we all agree on without mistakes it may have a few omissions as the one you mentioned. We plan to work on these deteails when Jones returns from Japan next Monday and submit a fianl report to the comittee.

Thanks for your comment and with best regards Moshe Gai.
~ Moshe Gai RLG2@WATSON 11/01/89*Interpretation of expt with J

Moshe also blames me (1/8/90 e-mail) for lack of timing between up & down counters: nonsense.

Moshe's faulty formula pre-dates October, and he should accept responsibility for it. — SE Jones

To: Steve Jones 1/8/90 (1)

From: Moshe Gai

Subject: Correcting Your Student's (Al Anderson) Mistakes

He used the following <u>wrong</u> formula:

$$P(K) = \binom{M}{K}\binom{D}{K} K!\, \varepsilon^K (1-D\varepsilon)^{M-K}$$

to correct it he needs to allow multiple hits in the detector, since our setup is blind to multiple hits. By summing over all combinations, while each multiple hit probability is small, a final large probability occurr.

so:

$$P(K) = \binom{M}{K}\binom{D}{K} K!\, \varepsilon^K \Big[\underbrace{(1-D\varepsilon)^{M-K}}_{\text{single hit}} + \underbrace{(1-D\varepsilon)^{M-K-1}}_{\text{double hit}} + \dots\Big]$$

In square bracket you will recognize a geometrical sum:

$$= \binom{M}{K}\binom{D}{K} K!\, \varepsilon^K \frac{1-(1-D\varepsilon)^{M-K}}{1-(1-D\varepsilon)}$$

$$= \binom{M}{K}\binom{D}{K} K!\, \varepsilon^K \frac{1-1+(M-K)D\varepsilon}{1-1+D\varepsilon}$$

$$= \binom{M}{K}\binom{D}{K} K!\, \varepsilon^K \cdot (M-K)$$

$$P(K) = \frac{M!}{(M-K)!}\,\frac{D!}{(D-K)!}\,\varepsilon^K\,\frac{(M-K)}{K!}$$

Note: Al is a senior researcher, and this formula is correct for only single hits, just as Al used it to check his Monte Carlo.

Notes on the
Derivation of the probability expression in the Yale 'Interim Report'
A. N. Anderson
11 Jan 1990

Introduction

The derivation of the expression used in the Yale 'Interim Report', for the probability $P_{M,D}(k)$ of getting k hits among D detectors of efficiency ϵ, when exposed to a burst of M neutrons, begins from the following expression:

$$P_{M,D}(k) = \binom{M}{k}\binom{D}{k} k!\,\epsilon^k \sum_{i=0}^{M-k} A_i \qquad (1)$$

where ϵ^k is the probability that k of the M neutrons each hit a detector, $k!$ is the number of different permutations of those k neutrons, and the $\binom{M}{k}$ and $\binom{D}{k}$ are the number of possible combinations of k from M neutrons and D detectors. The terms in the summation are the probabilities that the remaining $M-k$ neutrons hit exactly i of the previously hit detectors, and none of the others. The expression used for A is

$$A_i = (1-D\epsilon)^{M-k-i} \qquad (2)$$

by which the ith term is meant to be the probability that $M-k-i$ of the remaining neutrons miss all D detectors.

The summation is then evaluated as a geometric series yielding

$$P_{M,D}(k) = \binom{M}{k}\binom{D}{k} k!\,\epsilon^k \frac{1-(1-D\epsilon)^{M-k}}{1-(1-D\epsilon)} \qquad (3)$$

The approximation

$$(1-x)^M \approx 1 - Mx \qquad (4)$$

is invoked to yield

$$P_{M,D}(k) \approx \binom{M}{k}\binom{D}{k} k!\,\epsilon^k (M-k) \qquad (5)$$

which is essentially the expression used in the report. Actually their correction $(M-k)/k!$ should be $(M-k+1)/k!$ but that is a minor point, considering all the other errors in the derivation.

To: Steve Jones 1/8/90 (1)

From: Moshe Gai

Subject: Correcting Your Student's (Al Anderson) Mistakes

He used the following <u>wrong</u> formula:

$$P(K) = \binom{M}{K}\binom{D}{K} K!\, \varepsilon^{K} (1-D\varepsilon)^{M-K}$$

to correct it he needs to allow multiple hits in the detector, since our setup is blind to multiple hits. By summing over all combinations, while each multiple hit probability is small, a final large probability occurr.

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In square bracket you will recognize a geometrical sum:

$$= \binom{M}{K}\binom{D}{K} K!\, \varepsilon^{K} \frac{1-(1-D\varepsilon)^{M-K}}{1-(1-D\varepsilon)}$$

$$= \binom{M}{K}\binom{D}{K} K!\, \varepsilon^{K} \frac{1-1+(M-K)D\varepsilon}{1-1+D\varepsilon}$$

$$= \binom{M}{K}\binom{D}{K} K!\, \varepsilon^{K} \cdot (M-K)$$

$$P(K) = \frac{M!}{(M-K)!} \frac{D!}{(D-K)!} \varepsilon^{K} \frac{(M-K)}{K!}$$

by rearranging:

$$= M \cdot D\varepsilon * (M-1)(D-1)\varepsilon * \ldots * (M-K+1)(D-K+1)\varepsilon * \frac{M-K}{K!}$$

We now define (as in our DOE/ERAB report):

$$N \cdot \varepsilon = \varepsilon(N)$$

$$p(K) = M\,\varepsilon(D) * (M-1)\,\varepsilon(D-1) * \ldots * (M-K+1)\,\varepsilon(D-K+1) * \frac{(M-K)}{K!}$$

You will recognize here <u>our formula</u> from our report, <u>multiplied</u> by a factor $\frac{M-K}{K!}$

This factor in our case ($M=12$, $K=3$) is of the order unity and was neglected in the report.

<u>THERE IS NO MISTAKE IN THE REPORT!</u>

Consider an extreme case (Never encountered in our set up):

One detector ($D=1$) with $\varepsilon = 100\%$.

for $M=2$ our report formula gave hit probability of 200%. (loosely speaking a double hit...)

but $\frac{M-K}{K!} = \frac{1}{2}$ so $p = 100\%$.

for $M=3$ we get 300%, but $\frac{M-K}{K!} = \frac{1}{3}$

so again $p = 100\%$.

You will note that this agreement is in fact accidental, since in the geometrical sum that we performed $(1-D\varepsilon) = 0$.

Related info – from Moshe e-mail dated 8 Jan. 1990

In the example we have above with one detector with efficiency of 100% and a neutron source of multilicity 2, using my equation yield an efficiency of 200%. In this sense it is simply interpreted as 100% probability for a double hit. In fact you will notice that the formula we wrote in our report corresponds to the first binomial times K!*e**K, and the rest of the factors in (1) need to be rewritten and summed over. While having efficiency larger than 100% as suggested in my formula are indeed strange, they simply have to be explained (i.e. they are not wrong).

May I suggest that we discuss it so as to avoid all this waist of time on trivia.

Notes on the
Derivation of the probability expression in the Yale 'Interim Report'

A. N. Anderson

11 Jan 1990

Introduction

The derivation of the expression used in the Yale 'Interim Report', for the probability $P_{M,D}(k)$ of getting k hits among D detectors of efficiency ϵ, when exposed to a burst of M neutrons, begins from the following expression:

$$P_{M,D}(k) = \binom{M}{k}\binom{D}{k} k!\epsilon^k \sum_{i=0}^{M-k} A_i \tag{1}$$

where ϵ^k is the probability that k of the M neutrons each hit a detector, $k!$ is the number of different permutations of those k neutrons, and the $\binom{M}{k}$ and $\binom{D}{k}$ are the number of possible combinations of k from M neutrons and D detectors. The terms in the summation are the probabilities that the remaining $M-k$ neutrons hit exactly i of the previously hit detectors, and none of the others. The expression used for A is

$$A_i = (1 - D\epsilon)^{M-k-i} \tag{2}$$

by which the ith term is meant to be the probability that $M-k-i$ of the remaining neutrons miss all D detectors.

The summation is then evaluated as a geometric series yielding

$$P_{M,D}(k) = \binom{M}{k}\binom{D}{k} k!\epsilon^k \frac{1-(1-D\epsilon)^{M-k}}{1-(1-D\epsilon)} \tag{3}$$

The approximation

$$(1-x)^M \approx 1 - Mx \tag{4}$$

is invoked to yield

$$P_{M,D}(k) \approx \binom{M}{k}\binom{D}{k} k!\epsilon^k (M-k) \tag{5}$$

which is essentially the expression used in the report. Actually their correction $(M-k)/k!$ should be $(M-k+1)/k!$ but that is a minor point, considering all the other errors in the derivation.

Discussion

The starting point of the derivation is correct provided the A's are chosen properly. Requiring one and only one hit per detector replaces the summation with just its zeroth term, $A_0 = (1 - D\epsilon)^{M-k}$, which is the probability that all remaining $M - k$ neutrons miss all D detectors. This results in

$$P_{M,D}(k) = \binom{M}{k}\binom{D}{k} k!\epsilon^k (1 - D\epsilon)^{M-k}$$

which is the expression I used to check the MonteCarlo output in my report. It satisfies the requirements of probability in that it is always between 0 and 1 and it sums over k to a number between 0 and 1, namely the probability for having an event with no multiple hits.

It is easy to show, however, that equation (3) is not a valid probability. Simply let M become very large. There are only two terms containing M, namely $\binom{M}{k}$ and $1-(1-D\epsilon)^{M-k+1}$. The first term increases roughly as M^k, and the second term approaches 1, since $1 - D\epsilon$ is less than 1 and its power goes to zero. Therefore the expression can become greater than one and is not a probability.

One reason (3) is not a probability is that the terms do not contain factors for all the neutrons. To allow for other hits on detectors, the expression adds conditional probabilities that $M - k - i$ of the remaining neutrons also miss all D detectors. Unfortunately, that says nothing about whether the remaining i neutrons hit a detector. If they fail to hit a detector, they have already been included in the previous term and we are counting them again.

In order for probabilities to be addable, they must be disjoint. The ith term, however, is included in the $i - 1$st term if the ith neutron does not hit a detector. In order to make them disjoint, then, the i neutrons must hit a detector, and the ith term should be of the form

$$A_i \propto \epsilon^i (1 - D\epsilon)^{M-k-i}$$

where now there are factors for each neutron.

The magnitude of the error can be appreciated by the size of the correction, that is, each term has now been decreased by a factor of ϵ^i, which when summed over a large M, can make an enormous difference. It also means that the sum in (2) is no longer a geometric series, and hence the form of (3) is no longer implied. The new form has a much better chance of satisfying the probability conditions.

We are not done yet, however, because we have not counted up all the ways the i neutrons can hit the detectors, that is, we have not determined the coefficients of A_i. In general, there is much subtlety involved and it is very easy to make errors in determining these, and I do not choose to pursue this path further. Many seemingly plausible arguments have generated expressions which, when evaluated, normalized to more than 1, or which, when compared to a MonteCarlo for the same system, gave discrepant results. There is no reason to believe that the most simple system might not have an extremely complicated expression for the exact probability. It is at this juncture that a MonteCarlo, properly tested against simple cases, justifies greater confidence than a derived expression.

For the sake of completeness and to underscore my remark about subtlety, let me introduce an expression which has the advantages of legality, plausibility, and agreement with the MonteCarlo. It bears little superficial resemlance to (1), however:

$$P_{M,D}(k) = \binom{D}{k}(1-(1-\epsilon)^{M-k+1})^k(1-(D-k)\epsilon)^{M-k} \tag{6}$$

The second term represents the probability that each of k detectors was not missed by all $M-k+1$ neutrons. (The other $k-1$ neutrons presumably having been required to hit the other $k-1$ detectors. The last term is the probability that the $M-k$ neutrons not required for hits were precluded from hitting the remaining detectors. It has the nice property that as M becomes large, all probabilities go to zero except for $k = D$, which yields 1, as it should. Presumably a proper choice of A's would yield this result. It is difficult to prove that this expression is exactly correct, however, or that the correct expression might not be much more complicated.

Having shown that (3) is not a correct expression, I will mention also that approximation (4) is incorrect. The easiest way to see this is again to make M large. Whereas $(1 - D\epsilon)$ is always between 0 and 1, and raising to the power $M - K + 1$ preserves this property, note that the approximation $1 - (M - k + 1)D\epsilon$ is unbounded, and expression (5) becomes greater than one even for relatively small M.

It is necessary to dispose of just one more erroneous proposition, namely, that somehow inclusion of the $(M - k)/k!$ term restores legitimacy to (5). As I have already pointed out (5) is invalid even including the term in question. An example has been used in which $D = 1$ and ϵ is 100%. In this case k is constrained to be either 0 or 1 (It must be less than or equal to D), so $k! = 1$. In this case the term takes on the values 2 and 1 for $M = 2$, and 3 and 2 for $M = 3$. The term actually increases already absurdly large 'probabilities'. It should be obvious that this term does nothing to help (5).

Conclusion

It is not unreasonable that *anyone* might err in trying to derive an expression for the number of unconstrained detector hits. It is a somewhat subtle problem. What *is* unreasonable is that a Professor of Physics at Yale, who is ostensibly expert at experiments in which problems of this type occur regularly, should have so little understanding of elementary probability that not only does he fail to apply simple tests to his results to ensure their validity (or at least their plausibility), but that he does not even recognize such tests when someone else applies them, and even goes so far as to invent new interpretations of probability.

I hope enough has been demonstrated that there will be no necessity to pursue this point further.

From: IN%"GAI%YALEVM.BITNET@VM" "MOSHE GAI, (203)432 5195, FAX:(203)432 3522"
To: JONESSE@VANLAB.BYU.EDU
CC:
Subj: Your memo of 12/20

Received: from YVAX.BYU.EDU by VANLAB.BYU.EDU; Thu, 21 Dec 89 08:45 MST
Received: from JNET-DAEMON by YVAX.BYU.EDU; Thu, 21 Dec 89 08:45 MST
Received: From BYUVM(YMAIL) by BYUVAX with Jnet id 0397 for JONESSE@BYUVAX; Thu, 21 Dec 89 08:45 MST
Received: from YaleVM.YCC.Yale.Edu by VM.BYU.EDU (Mailer R2.05) with BSMTP id 6347; Thu, 21 Dec 89 08:44:10 MST
Received: by YALEVM (Mailer R2.03B) id 8997; Thu, 21 Dec 89 10:40:45 EST
Date: Thu, 21 Dec 89 09:36:41 EST
From: "MOSHE GAI, (203)432 5195, FAX:(203)432 3522" <GAI%YALEVM.BITNET@VM>
Subject: Your memo of 12/20
To: Steve Jones <jonesse@YVAX>
X-VMS-To: Steve Jones <jonesse@YVAX.BYU.EDU>
Resent-date: Thu, 21 Dec 89 08:45 MST
Resent-to: JONESSE@VANLAB.BYU.EDU
Resent-message-id: <06A8DD0B20DF0020A9@YVAX.BYU.EDU>
Message-id: <06A8E8D0BD3F000CA5@YVAX.BYU.EDU>
X-Envelope-to: JONESSE@VANLAB.BYU.EDU

Exchange on Menlove & origin of erroneous "2-4 bursts" expected at Yale to which I responded! (still in Yale Report, ignoring me).

Dear Steve,

Thanks for the memo. Good that we agree that the time for Yale-BYU-BNL experiment should be increased from 77 hours to 103. We are making progress, even if it is slow. I would still request the following calrified:

1. Since you talk about cylinder*hour, should you multiply the Yale time by 4 to account for the 4 cylinders? – already in ratio of sample masses

2. If indeed at Los Alamos you spent a long time for cracking the surface of the oxyde layer, should you not multiply the time we spent on the treated Ti662 by the appropriate factor to allow for the fact that these samples in our experiment are already pretreated and the deuterided already formed?

~~only 2 + 3 possibly~~ not Ti662

3. In the Menlove paper in Table III, I count 27 bursts, and you told the DOE that you have a grand total of 37 bursts. So in fact the data in the Menlove paper is about 75% of your data. We than can discuss these published material.

includes electrolysis & ≤ 20 n°/burst
actually 12 of ≥ 20 n° in Menlove

For the Menlove data I find:

A. All cylinder bursts during cycle 5 or 9 or smaller (i.e. before three days of running). Where is the evidence that one needs to wait for a long time? You even have 11 bursts at cycles 1, 2, 3 and 4, i.e. within a day or two. The Ti662 sample bursts within cycle 3, 4!! Again where is the evidence that we need to wait weeks??

wrong normalization! who says?
– given time!

B. Since ~~the~~ Menlove result ~~suggest 30-50%~~ chance per cylinder and since we used 4 cylinders twice, i.e. 2 to 4 bursts should have been obserevd, by us. – if enough time & efficiency!

origin of 2-4 bursts at Yale: wrong

C. If you insist that no background is included in the 13,000 hours than before I agree to that number I would require a detailed list of all experiment performed. Before I can calculate this number by myself, I will not put it in our paper. During the first period of the Menlove experiment you rotated between a dummy and a real cell, i.e. measured background half of the time. So you either should not multiply this time by 4 (four cylinders) or appropriately subtract the background time.

12/39 = 0.3

I just made the following observation:
While the Menlove paper covers a period of two months, it includes ~~75%~~ of the

bursts. Than for the rest of the five months you only had 10 bursts, i.e. 25% of the grand total? Steve it does not seem to me that you have improved the situation, in fact I would say that it got worst... NO.

Best Regards Moshe Gai.

Jan. 4, 1989

Dear Moshe,

I have returned from a family trip for the holidays and can address your questions. I should also say that our report on the analysis will be sent tomorrow by express carrier. It raises a number of questions which I hope you will address.

I will answer your questions point by point.

1. No, the normalization is done by mass, not number of cylinders. That is, we ran at Yale for 103 hours with about 200g of material per hour. At Los Alamos, the 13,000 hours were estimated based on the number of cylinders, each containing about 100 g of material. So the appropriate scaling is the ratio of (103 X 200) / (13,000 X 100), as I have said before.

2. None of the Ti662 samples used at Yale were "pretreated and the deuteride already formed." When we loaded the deuterium gas, the pressure dropped very little showing that very little deuteriding was occurring. (The used cathode materials did rapidly form a deuteride at Yale, but not the Ti662 samples.)

3. Your count of 27 bursts in the original Menlove paper includes 6 bursts from electrolysis experiments as well as bursts smaller than 20 neutrons. My count of 39 bursts is for gas-loading experiments only and for bursts of 20 or more neutrons. The Menlove Update shows the sizes of bursts in Table II, so that in the data set relevant to the submission to **Nature**, I count 12 bursts of 20 or more neutrons. So in fact the Menlove original paper represents about 30% (12/39) of the data which is consistent with the time periods involved (this answers your concluding remark.)

3A. You state that "For the Menlove data I find: A. All cylinder bursts during cycle 5 or smaller (i.e. before three days of running)." However, the original Menlove paper in fact shows bursts through cycle 9 and a typical cycle at LANL requires a full day so this would correspond to roughly 9 days of running (where did you get "before three days"?). Some runs do produce bursts during the first cycle or two. But statistically, we find as mentioned in the Menlove October update: "A time interval of several days is [often] required between the gas loading and the first neutron yield from the LN cycles." Thus, at LANL running for 14 cycles is not uncommon.

3B. About 30-50% chance per cylinder depends on long measurement times as I have said before. Also, to see 2-4 bursts at Yale assumes burst-detection efficiency comparable to that of LANL. Our analysis shows that this is not at all the case-- this is in the report in detail. This low efficiency is the major problem in the Yale set-up in my judgment, rather than the short running time, as we must discuss when you study our report carefully.

3C. We could ask Ed Garcia for a more detailed list of "all experiment performed" as you now say. I think the estimate of 13,000 hours suffices when the revised efficiency of the Yale set-up is taken into consideration--let's discuss this when you have read the report. We scale by the sample mass rather than by the number of cylinders as discussed above, and the background has been handled appropriately (i.e., not included in the count of 13,000 foreground hours).

Sincerely,

Steve

From: IN%"GAI%YALEVM.BITNET@VM" "MOSHE GAI, (203)432 5195, FAX:(203)432 3522"
To: JONESSE@VANLAB.BYU.EDU
CC:
Subj: A reply to your note on Jan. 4th

Received: from YVAX.BYU.EDU by VANLAB.BYU.EDU; Fri, 5 Jan 90 11:48 MST
Received: from JNET-DAEMON by YVAX.BYU.EDU; Fri, 5 Jan 90 11:47 MST
Received: From BYUVM(YMAIL) by BYUVAX with Jnet id 3881 for JONESSE@BYUVAX; Fri, 5 Jan 90 11:47 MST
Received: from YaleVM.YCC.Yale.Edu by VM.BYU.EDU (Mailer R2.05) with BSMTP id 5373; Fri, 05 Jan 90 11:46:37 MST
Received: by YALEVM (Mailer R2.03B) id 8329; Fri, 05 Jan 90 13:18:25 EST
Date: Fri, 05 Jan 90 12:30:50 EST
From: "MOSHE GAI, (203)432 5195, FAX:(203)432 3522" <GAI%YALEVM.BITNET@VM>
Subject: A reply to your note on Jan. 4th
To: Steve Jones <jonesse@YVAX>, kelvin lynn <kgl%APSEDOFF.BITNET@VM>
X-VMS-To: Steve Jones <jonesse@YVAX.BYU.EDU>, kelvin lynn <kgl@APSEDOFF.BITNET>
Resent-date: Fri, 5 Jan 90 11:48 MST
Resent-to: JONESSE@VANLAB.BYU.EDU
Resent-message-id: <FAC5DD8B05FF003AE9@YVAX.BYU.EDU>
Message-id: <FAC5E55558DF000CA5@YVAX.BYU.EDU>
X-Envelope-to: JONESSE@VANLAB.BYU.EDU

Dear Steve,

Thanks for your FAX note of Jan. 4th. Before replying to it let me inform you that I talked to Dave Lindley and he is pretty upset that you claim that he misquoted you in the Nature note on Nov. 9th. I would then like to have a conference call involving all three of us so that we can get to the bottom of this.

I make no such claim!

concerning your note:

I used mass!

point 1: While for DOE and to me you quoted a burst chance per cylinder, you now normalize by weight? Which method are you eventually going to use? In the Menlove paper I see an average weight of 55 grams per cylinder, why did you use 100 grams? For the Yale data you use real time 103 hours, but for the Los Alamos data you multiply the hours by the number of cylinders used in the four detector systems. We had our four cylinders in one detector system, and again why have you omitted the factor of 4? I am afraid I will have to insist on receiving a detailed account of all experiments performed in Los Alamos including: cylinder weights, time, etc. before I agree to the 13,000 cylinder*hours estimate.

MASS (repeat)

point 2: I shall remind you that on October 31st I FAXed to you comments on our paper by Kurt Zilm. I felt that the comments directly criticized your experiment at Los Alamos and thus should have your approval. Since we did not hear from you they are still not included in the improvements to the report. The comment read: "Before the Ti662 alloys used in the majority of these experiments were loaded into the pressure cells they were cleaned according to the protocol developed at Los Alamos. This was done to ensure that our samples were as close as possible in composition to those investigated in Ref. 2 (Menlove). Since the oxide layer on the alloys is not removed in this treatment it is higly unlikely that a significant amount of deuterium is incorporate into the metal lattice...". and as you write in your point 2, Kurt Zilm adds: "The sample in set E which contained used Pd electrodes did have a significant amount of deuterium...". So in fact what was originally a severe criticism of your experiment at Los Alamos (by our material science people) is now turn around by you as a cricism of the Yale experiment?

never received

No!

this is out of paper - summer's data corrected this

point 3: It is true that 6 bursts are from electrolysis samples. But I shall repeat, in your paper with Menlove you explicitly write: "the neutron emission always ceased after a few cycles". and before that: " A given cylinder was put through 7 to 14 of these cycles". Each cycle lasted about a day with some 5 hours (so the number of days is a bit smaller than the number of cycles). We

more during summer

not on all samples!

in fact run 12 cycles, just like Menlove. Do you still stand by the above statement of your paper? but up to ~ 24 cycles

point 3B: Let me state for the record you are now claiming that: "Also, to see 2-4 bursts at Yale assumes burst-detection efficiency comparable to that of LANL. Our analysis shows that this is not at all the case-- this in the report in detail. This low efficiency is the major problem in the Yale experiment set-up in my judgement, rather that the short running time, as we must discuss when you study our report carefully". Let us make it very clear you are now taking a new path in your attempt to kill the Yale experiment. I am looking forward to receiving your study of the burst detection efficiency of the Yale experiment. I hope you realize that calculating the efficiency is detailed, and just make sure you include all the factors before you mail to me your report. In fact you should consult our report where we quote the formula for the efficiency on p. 5. I noticed that Menlove in his letter to me made a mistake in calculating the efficiency of our experiment, and just make sure that you don't make the same mistake...

I wish to do it right Moshe!

I have!

ooooh...

With Best Regards Moshe Gai.

1/5/90

From: IN%"RLG2%WATSON.BITNET@VM" 19-JAN-1990 09:52:25.68
To: JONESSE@VANLAB.BYU.EDU
CC:
Subj: Retransmission.

Received: from YVAX.BYU.EDU by VANLAB.BYU.EDU; Fri, 19 Jan 90 07:53 MST
Received: from JNET-DAEMON by YVAX.BYU.EDU; Fri, 19 Jan 90 07:52 MST
Received: From YKTVMV(RLG2) by BYUVAX with Jnet id 8416 for JONESSE@BYUVAX;
Fri, 19 Jan 90 07:52 MST
Resent-date: Fri, 19 Jan 90 07:52 MST
Date: 19 January 1990, 06:59:03 EST
From: RLG2%YKTVMV.BITNET@VM
Subject: Retransmission.
Resent-to: JONESSE@VANLAB.BYU.EDU
To: JONESSE@YVAX
Reply-to: RLG2%WATSON.BITNET@VM
Resent-message-id: <EFE66D178F3F200733@YVAX.BYU.EDU>
Message-id: <EFE678E9609F2000BA@YVAX.BYU.EDU>
X-Envelope-to: JONESSE
X-VMS-To: JONESSE@YVAX.BYU.EDU

==
Date: 18 January 1990, 22:07:07 EST
From: (R.L.Garwin (914) 945-2555) RLG2 at YKTVMV
IBM Fellow and Science Advisor to the Director of Research
P.O. Box 218
Yorktown Hts, NY 10598
To: GAI at YALEVM
cc: JONESS at BYUVAX
Subject: NOTEs of 01/18/90.
Reply-To: RLG2 at WATSON

Thanks for the NOTES, received fine via RLG2 at WATSON. I was most interested in your letter to Howard Menlove, and dismayed that you have not yet received a response.

Certainly his criticisms of (a) your "low efficiency" and (b) your inadequate running time are unwarranted. I will be most interested in any data Howard has obtained (ALL data, in fact) while running in caves or under other circumstances. ??

Indeed, when I pointed out that your formula could not be "CORRECT" I did not mean that it was really misleading in the range where it would be used-- that is, for efficiencies substantially less than 50% or so. And it is never VERY far off. ? have you checked?

I would be grateful if you would put copies of the references into the mail for me-- it will save me a good deal of time in the library.

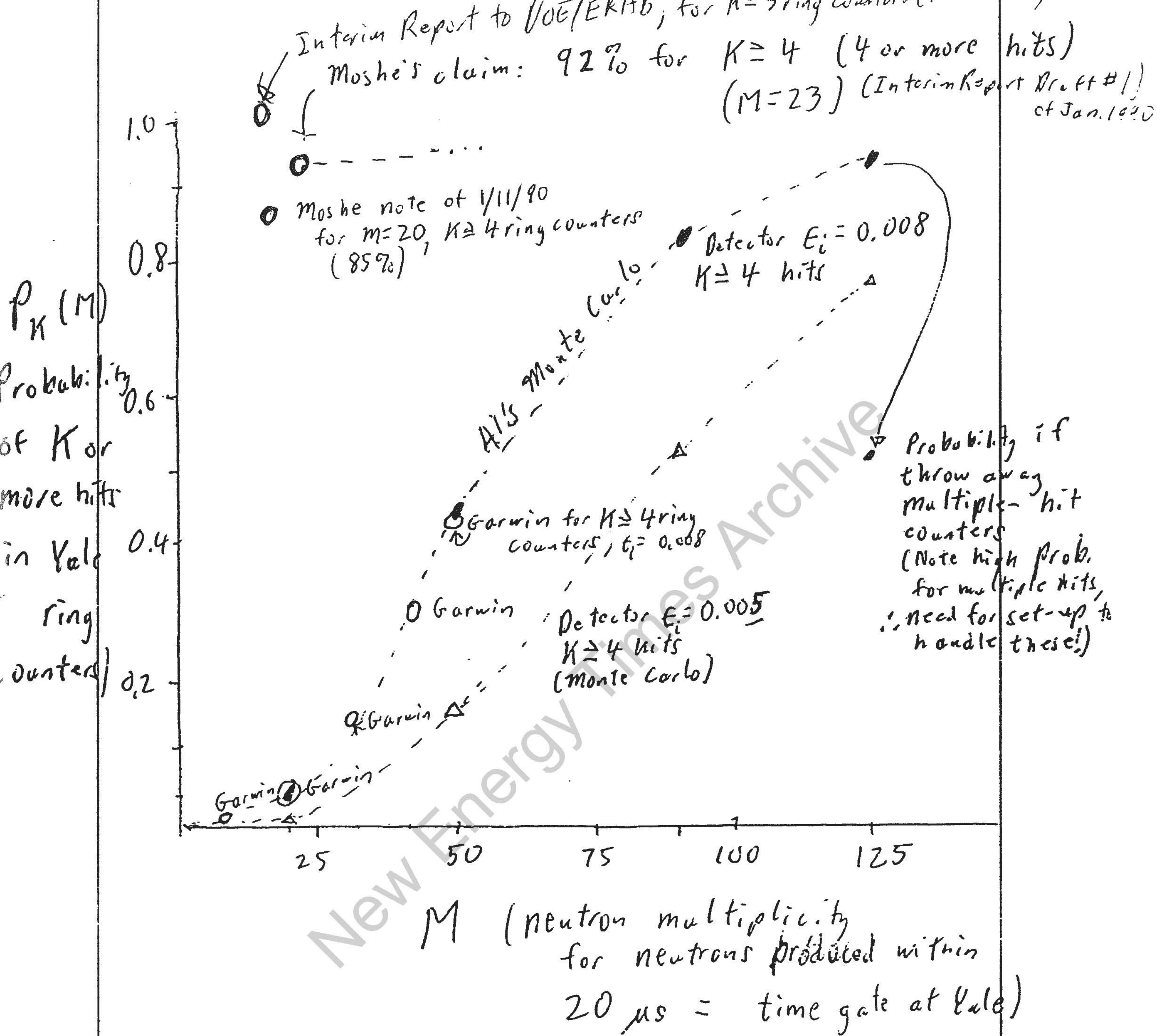
Thanks VERY much for the information.

Dick Garwin

Received: by YALEVM (Mailer R2.03B) id 0184; Thu, 18 Jan 90 23:34:28 EST
Date: Thu, 18 Jan 90 23:33:02 EST
From: "MOSHE GAI, (203)432 5195, FAX:(203)432 3522" <GAI@YALEVM>
Subject: Re: NOTEs of 01/18/90.
To: RLG2@WATSON
In-Reply-To: Message of 18 January 1990, 22:07:07 EST from <RLG2@YKTVMV>

Please note that Jones' e-mail is JONESSE @ BYUVAX (your cc has JONESS...)

Jones 1/~~19~~ 26/90



Probability of 4 or more hits in Yale ring counters; contrasting Moshe's calculations and Anderson's Monte Carlo Calculations. Note that if the Monte Carlo is accurate, then the Yale experiment was not set up well to detect bursts.

From: IN%"GAI%YALEVM.BITNET@VM" "MOSHE GAI, (203)432 5195, FAX:(203)432 3522"
To: JONESSE@VANLAB.BYU.EDU
CC:
Subj:

Dear Kelvin and Steve,

I have written draft number 2 of our paper that includes answers to all BYU's questions, including the exact solution of hit probabilities and the various approximation that can be used. Note that in draft two I only take the exact Mottelson solution, without any multiple scattering (one neutron hits two detectors) or double hits (two neutrons hit one detector). This was done in draft number 1. Since I feel we can easily agree on Mottelson's formalism, and in order to expedite publication I am willing to stick to the most simple case, even though multiplee scattering and double hits make our efficiencies even larger. Remember the probability for such events in each detector are small, but when you work out the combinatoric for the whole array, it makes a difference. I have FAXed a copy of these calculations to BYU last week and I am FAXing one to you Kelvin now. Really?

I would be happy to add any more answers to unresolved questions in future drafts. I would think that the differences between BYU and Yale are now so small that the whole discussion can be resolved within the collaboration, and we should proceed to publication. I will ~~however,~~ have no objection to arbitration! I would only hope that the embarrasment caused by the letter from Dick Garwin (cc to Jones) would bring the voice of reason. K

Best Regards Moshe gai. (Knuckle under, Jones)

Received: from YVAX.BYU.EDU by VANLAB.BYU.EDU; Mon, 22 Jan 90 07:43 MST
Received: from JNET-DAEMON by YVAX.BYU.EDU; Mon, 22 Jan 90 07:43 MST
Received: From BYUVM(YMAIL) by BYUVAX with Jnet id 2184 for JONESSE@BYUVAX; Mon, 22 Jan 90 07:43 MST
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Received: by YALEVM (Mailer R2.03B) id 6012; Mon, 22 Jan 90 09:41:54 EST
Resent-date: Mon, 22 Jan 90 07:43 MST
Date: Mon, 22 Jan 90 09:23:36 EST
From: "MOSHE GAI, (203)432 5195, FAX:(203)432 3522" <GAI%YALEVM.BITNET@VM>
Resent-to: JONESSE@VANLAB.BYU.EDU
To: kelvin lynn <kgl%APSEDOFF.BITNET@VM>, Steve Jones <jonesse@YVAX>
Resent-message-id: <ED8C348D421F202FE5@YVAX.BYU.EDU>
Message-id: <ED8C3DCBE57F2000BA@YVAX.BYU.EDU>
X-Envelope-to: JONESSE
X-VMS-To: kelvin lynn <kgl@APSEDOFF.BITNET>, Steve Jones <jonesse@YVAX.BYU.EDU>

From: IN%"GAI%YALEVM.BITNET@VM" "MOSHE GAI, (203)432 5195, FAX:(203)432 3522"
To: JONESSE@VANLAB.BYU.EDU
CC:
Subj: Cleaner analysis of the data

BRIGHAM YOUNG
UNIVERSITY

THE GLORY OF GOD
IS INTELLIGENCE

Dear Steve and Kelvin,

In the data analysis for the interim report we used only one dimensional gates on the pulse shape spectra. Ralph has also performed the analysis with gates on the two dimensional pulse shape spectra. I discussed these results with you Steve over a month ago, and we both know that this more conservative analysis yields an amazingly clean spectra. We are now going to incorporate in Draft number 2 of our paper the results of this analysis, too. The results are embarrasingly clean, even cleaner than what we have gotten so far!!

what?

we do?! How about Al's doubles?

Best Regards Moshe Gai.

Received: from YVAX.BYU.EDU by VANLAB.BYU.EDU; Mon, 22 Jan 90 11:09 MST
Received: from JNET-DAEMON by YVAX.BYU.EDU; Mon, 22 Jan 90 11:09 MST
Received: From BYUVM(YMAIL) by BYUVAX with Jnet id 3709 for JONESSE@BYUVAX; Mon, 22 Jan 90 11:09 MST
Received: from YaleVM.YCC.Yale.Edu by VM.BYU.EDU (Mailer R2.05) with BSMTP id 9945; Mon, 22 Jan 90 11:09:08 MST
Received: by YALEVM (Mailer R2.03B) id 1140; Mon, 22 Jan 90 13:08:46 EST
Resent-date: Mon, 22 Jan 90 11:09 MST
Date: Mon, 22 Jan 90 13:01:05 EST
From: "MOSHE GAI, (203)432 5195, FAX:(203)432 3522" <GAI%YALEVM.BITNET@VM>
Subject: Cleaner analysis of the data
Resent-to: JONESSE@VANLAB.BYU.EDU
To: Steve Jones <jonesse@YVAX>, kelvin lynn <kgl%APSEDOFF.BITNET@VM>
Resent-message-id: <ED6F68AF61DF2033D7@YVAX.BYU.EDU>
Message-id: <ED6F715E967F2000BA@YVAX.BYU.EDU>
X-Envelope-to: JONESSE
X-VMS-To: Steve Jones <jonesse@YVAX.BYU.EDU>, kelvin lynn <kgl@APSEDOFF.BITNET>

DEPARTMENT OF PHYSICS AND ASTRONOMY
[illegible]
[illegible]
PROVO, UTAH 84602
(801) 378-4361

January 18, 1990
Dear Moshe,

Quoting authorities (G. Hagemann, O. Andersen, L. Westerberg) to justify your hit probability estimates is not sufficient -- your work must be correct. You mention that your formula is an **approximation** which immediately should raise worries, but Al shows in detail where you went wrong (11 Jan. memo) -- please study and respond. I am truly amazed to read in your memorandum of yesterday: "I shall state again, there is nothing wrong with Moshe's formula as appeared in the [interim] report."

In the latest Report, you state "we estimate for multiplicity M > 22, at least 92% hit probability of 4 or more neutrons in the ring counters (i.e., $P4(22)+\ldots+p10(22) > 92\%$). Hence, from our data we deduce an upper limit on neutron bursts from our cells, to include less than 22 neutrons with 90% confidence." The conclusion is terribly misleading since the hit probability on which it depends is way wrong (just as in the original Interim Report).

I wish you would respond to Al's detailed discussions and Monte Carlo calculations regarding hit probabilities. I have summarized in Fig. 1 the hit probabilities for 4 or more detected neutrons in the ring counters [P4 (M)]. Notice, for example, that the probability P4 for M=23 neutrons is about 5%, not 92% as you obtained. That is an enormous difference and drastically affects conclusions obtainable from the experiment. Al's probability for one or more ring counters to be hit is about 80% (Al's tables for M=20 actually), which is still less than your 92% for four hits.

Is your value reasonable? A simple calculation suffices to show that it is not:

$$23 \text{ source neutrons} \times 0.0008 \text{ (efficiency/ring counter)} \times 10 \text{ ring counters} = 1.8 \text{ expected hits.}$$

But then the probability of detecting 4 or more neutrons must be considerably less than the 92% quoted in your latest report! Your approximate formula evidently breaks down even for small multiplicities like this and thus has lead to misleading conclusions as we argued at length in our Jan. 3 and subsequent reports. (Al even shows [Jan. 11 memo] where your expression double-counts neutrons; you have not responded.) You will find that the expectation value of about 1.6 hits for 20 source neutrons comes out of Al's Monte Carlo calculation just as expected (Al's report of 6 Jan., Appendix B).

The fact that your formula checks for M=1 (as quoted in the latest report) is hardly convincing -- the probabilities are already far too high for M=23!

In addition to this gross error, the quoted efficiencies of 0.8%

per ring counter are suspect. We have run a Monte Carlo for the approximate geometry of the Yale set-up and get significantly smaller values. (Remember, if you measure the efficiency with a source, a simple one-dimensional pulse-height/pulse-shape gate on neutrons will lead to inaccurate results.) Al shows in his Jan. 6 report that serious electronic inefficiencies are evident (e.g., counters U3 and D1). For these reasons, I have included on Fig. 1 the probabilities obtained assuming ring detector efficiencies of 0.5% which may still be too high. Notice how insensitive the set-up is to bursts of even 50 neutrons!

I find that my challenge of the hit probability statements in the Interim Report and the latest version corresponds quite closely with Richard Garwin's challenge (Nov. 1 letter to you, which I learned about only a few days ago). And I will ask just as he did then:

"I would like to receive from you a corrected version of the manuscript which does not have this misleading statement, and which does the analysis right."

Be sure to fold in the correct geometrical and electronic efficiencies for each counter. And include in the analysis the (poor) handling of multiple hits in counters in the Yale set-up-- the Monte Carlo calculations show that these are not of low probability as erroneously stated in both the Interim and latest reports. Please do not ignore the observed excess of hits in two ring counters in foreground data versus background (Al's Table 1, Jan. 6 report). Finally, when comparing with Menlove, be sure to include the fact that the Yale set-up is sensitive to bursts for only 20 μs, while Menlove is sensitive for up to 128 μs. Bursts could be quite long and spread out -- a recent burst candidate of 170 neutrons detected here lasted about 150 μs.

Al and I have provided lengthy documents which attempt to do the analysis right, and we feel that your responses do not adequately address the concerns we have legitimately raised. This is particularly true of our hit probability calculations. The latest draft of the Interim Report incorporates nothing of our analyses in it that I can see. This is not acceptable.

I find myself repeating, repeating, repeating what I have said in past memos and reports. Look, I really think the best thing to do is to admit that the Yale set-up was far from optimal for neutron burst detection and to do the experiment right based on we have learned, THEN publish. We can make an apparatus MUCH more sensitive for burst detection and do the definitive job we set out to do (whether we see bursts or not). In my January 3 letter to the collaboration, I suggested this solution. I think it is far better than rushing to publish based on the set-up of last August.

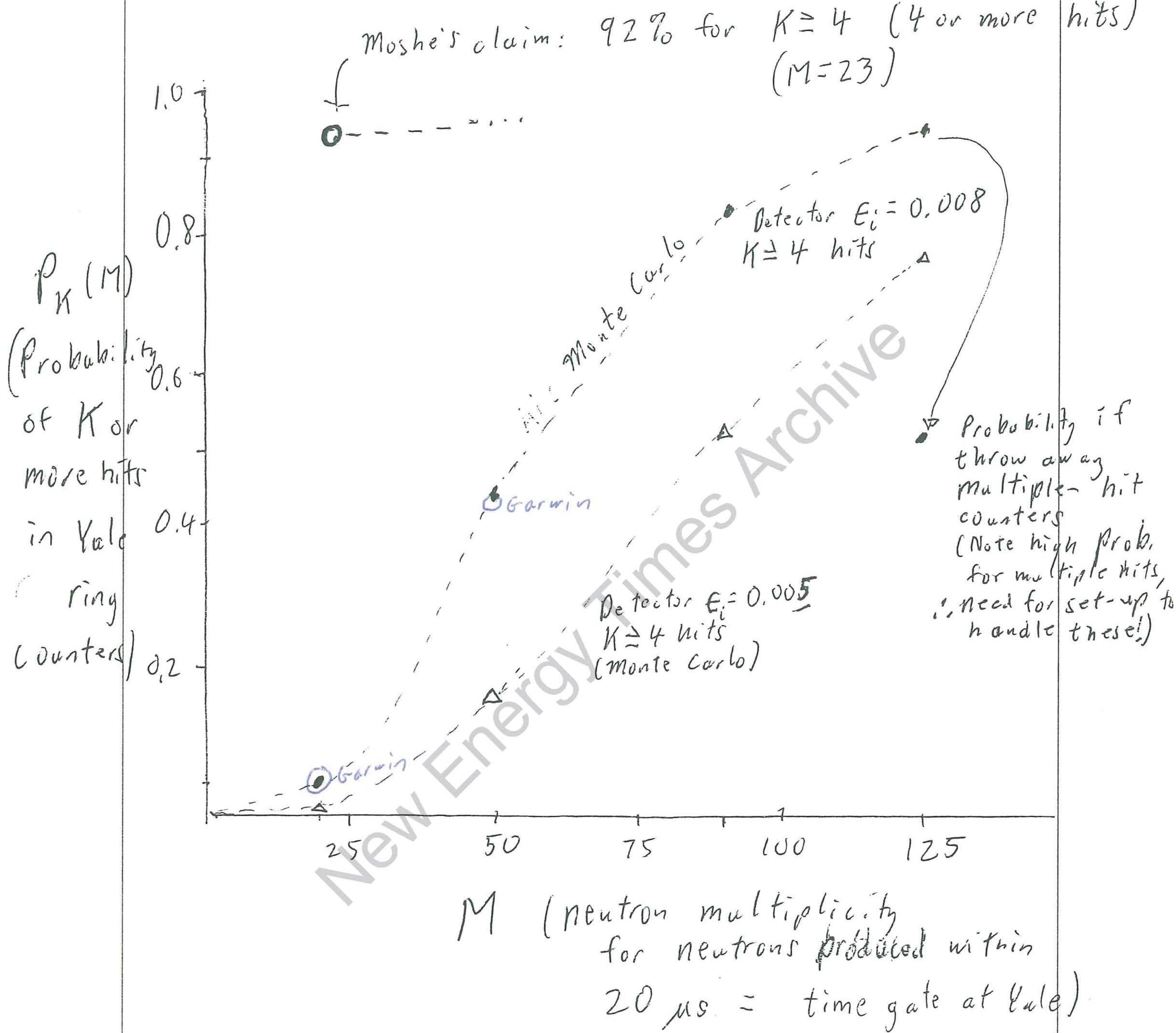
With best wishes to a stubborn associate,

Steven E. Jones

cc: associates

Sending material to R. Garwin etc. to end this argument hopefully!

Jones 1/19/90



Probability of 4 or more hits in Yale ring counters, contrasting Moshe's calculations and Anderson's Monte Carlo Calculations. Note that if the Monte Carlo is accurate, then the Yale experiment was not set up well to detect bursts.

1/15/90: Latest version of 'Interim Report' (by Moshe)

event. Since the probability for a double hit in a ring counter is estimated to be smaller than 3%, we conclude that all events are consistent with background events, with 97% confidence.

WRONG!

Inspecting the time scale and energy deposited in detectors U5, U1 and U0 in the event of Run 45, we may suggest that the event is consistent with one high energy (E > 7.3 MeV) cosmic-neutron (arriving from the less shielded front side, see Fig. 1), that undergoes triple scattering, including one large angle scattering from detector U5 to its immediate neighbor detector U1, and then followed by a small angle scattering into the central detector U0 lying below the ring detectors (thus yielding to negative time of flight).

We have calculated the hit probability in our array using the formalism first developed by B.R. Mottelson and discussed in Ref(s) 4. The hit probability is essentially determined by the array of ring detectors. In Fig. 5 we show the hit probability $-P_K(M)$, for the ring counters only, as a function of multiplicity (M), for different ~~folds~~ (K). Note that $P_1(1)$ -the hit probability for one neutron in any one ~~of the ten~~ ring counters, is 8%, as expected. We estimate for multiplicity M>22, at least 92% hit probability of 4 or more neutrons in the ring counters (i.e $P_4(22)+ \ldots +P_{10}(22) > 92\%$). Hence, from our data we deduce an upper limit on neutron bursts from our cells, to include less than 22 neutrons with 90% confidence. Using the data of Menlove et al. [2], we expect some 2-6 neutron bursts, including more than 20 neutrons each, over the duration of our experiment.

Wrong!

In Fig. 6 we show the rate above background of neutrons emitted randomly. For these data the time of flight coincidence method was used [3], that yielded a low background of 2 cph (0.4 cph in upper hemisphere

→ Cold fusion box file

Response to notes from Dr. Garwin on Probability Calclulations

A. N. Anderson

25 January, 1990

There were some minor differences between my calculations and Dr. Garwin's, in assumptions about the center counters, but the results are comparable for situations ignoring the center counters. Probabilities for 2-or-more and 4-or-more ring hits with bursts of 20 and 50 neutrons are presented below:

Neutrons	Hits	Garwin	Anderson
20	2	.4478	.4569
20	4	.0477	.0432
50	2	.8916	.9045
50	4	.4308	.4386

Each case assumes a ring counter efficiency of 0.008. The 'Anderson' values are taken from the 'ignore;any' column of Appendix B. of my original report. I feel this represents good agreement, especially considering that Dr. Garwin is using an algebraic expression and I was using a MonteCarlo. Considering the agreement thus obtained I see no reason to beat this particular horse further.

Received: by YALEVM (Mailer R2.03B) id 5544; Tue, 30 Jan 90 20:42:04 EST
Date: Tue, 30 Jan 90 20:28:42 EST
From: "MOSHE GAI, (203)432 5195, FAX:(203)432 3522" <GAI@YALEVM>
To: "R.L. Garwin" <rlg2@watson>

Thanks for your note of 1/30/90.

I received today a letter (in normal mail) from you including a copy of your communication to me, Jones communication to you and A COPY of the Menlove paper to the NSF/EPRI meeting. Please do not send it again by Express mail.

The paper of Menlove et al. is very helpful. For some time I was trying to get data on bursts statistics from either Jones or Menlove, without much luck. I find quite a few useful points in this paper, which I was completely unaware of (in spite of repeated questions to Jones on the data). Using these data I estimate 3-5 bursts for our experiment, based on the Menlove rates.

I would be very interested in hearing what Steve Jones tells you about a future publication of our experiment. I am surprised that he finds the Los Alamos data convincing, and our experiment unpublishable. I would be aminable to publishing the data without Jones, but I would hate to spend the rest of my time discussing Jones criticism of the Yale experiment, which amounts to a few % effect on the sensitivity of the quoted upper limit. In other word we all agree that we have not seen any burst, the question is only at what level of sensitivity do we rule out the Los Alamos results.

Best Regards Moshe Gai.

UNIVERSITY OF
ROCHESTER

8 JAN 90 13. 48

-R.L. GARWIN-

1A

COLLEGE OF ARTS AND SCIENCE
DEPARTMENT OF CHEMISTRY

John R. Huizenga
Tracy H. Harris Professor

January 3, 1990

Dr. Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598

Dear Dick:

I appreciate that you have sent me several relevant pieces of information on cold fusion since the last meeting of our Panel on October 31. Since I'm attempting to keep an up-to-date file on cold fusion, I would appreciate any new information that comes across your desk.

The final report of the Panel has now been duplicated and you should get a copy soon. It took some three weeks beyond October 31 to remove all the typographical errors and make minor changes, however, most of the delay has been due to DOE's reproduction schedule.

Happy New Year.

Sincerely,

John

John R. Huizenga

JRH:dsm

P.S. I'm enclosing a letter I received today from Bockris.

467 Hutchison Hall
University of Rochester
Rochester, New York 14627
(716) 275-4217

010390.JRH

TEXAS A&M UNIVERSITY
DEPARTMENT OF CHEMISTRY
COLLEGE STATION, TEXAS 77843-3255

December 26, 1989

(409) 845-5335
FAX(409) 845-4205

Dr. John Huizenga
Co-Chairman
Professor of Nuclear Engineering
Department of Chemistry
University of Rochester
Rochester, NY 14627

Dear Dr. Huizenga:

I am now writing to you in respect to the present position of experimental work on cold fusion and its connection to the ERAB report on that subject.

With the recent publications describing nuclear electrochemical effects from Oak Ridge, Los Alamos, Brookhaven, and N.R.C., the Fleischmann-Pons phenomena have now been repeatedly confirmed by U.S. Government Research workers. These men have added their confirmatory experiments to those obtained from April onwards at Texas A&M, Stanford, Minnesota and elsewhere. Apart from the U.S. Government labs and universities, these startling phenomena have been observed and reported on in many other countries.

?? papers

As to those expert and respected scientists (e.g., at Yale, MIT, Cal Tech, etc.) who denied the existence of these effects, it is now easy to understand the absence of their results: the effects, now seen by so many, do not switch on for many weeks of electrolysis. As to reproducibility, it is poor. However, for 1 mm wires, we have a 70% chance of producing tritium, if electrolysis is continued for more than 10 weeks..

The situation as now seen appears markedly contrary to the impression engendered by the report of your committee. It would be fair to say that many who read it might have thought that nuclear electrochemistry was perhaps the giant mistake which it is universally pictured to be among those who do not research it.

In view of the direct confirmation of these phenomena from so many sources, I put it to you that the negative ERAB report on Cold Fusion should be withdrawn. As an official U.S. Government document, it is important that it be beyond reproach (and above all, not subject to ridicule). The recent U.S. government lab publications are contrary to the general drift of its content.

In support of this contention, relatively large amounts of money (EPRI, \$2 M, U.S. Government Labs ~ \$1 M, Japanese MITS Group c \$5 M[1]) are slated to be spent in 1990 in the investigation of Cold Fusion. Even the Indian Government has decided to fund more than 50 research workers in this area.

The ERAB reports recommendation against large scale funding will fetter the U.S. competitive position in the development of a field which may, in 10-20 years, be seen as more important than high T superconductivity. At present, the Hot Fusion research budget is around \$320 M (with total U.S. spending on fusion said to be ~ \$1 B/year). I wrote a chapter in a book on this subject in 1980 and recently discussed with a number of scientists active in this field. Both my 1980 analysis (where I showed that approach to the Lawson criterion had become assymptotic) and the opinions expressed to me in 1989, suggested that no economic energy source from hot fusion is in sight. Those with whom I discussed the matter agreed that even if one far off day sustainment should be achieved, the cost of the resulting energy would be far more than that from fission.

These matters, then, stress the need for a radical revision of the ERAB Report. The Great Problem of Fusion - sustainment - has been now achieved under the influence of electric fields in place of high temperatures. It is true that the achievement is halting and as yet unsatisfactory, but the energy density of the best results is that of a nuclear fission reactor - and fusion (i.e., tritium production) has been sustained (in occasional successful experiments) over many weeks.

I put it to you that the first and necessary step is that the negative ERAB Report, which , in the light new research findings, no longer corresponds to the new findings, should be formally withdrawn. After four or five years of good research funding may be the time to write a new report.

I write this letter to you not in the sense of an adversarial challenge but once which asks a sober and relaxed reconsideration in a subject which may eventually have an importance greater than that seen within DOE circles for hot fusion.

With the compliments of the season,

Sincerely,

J. O'M. Bockris

JOMB/jl

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[1]Estimated from statements made to me by leading Japanese electrochemists in Kyoto, September 1989. It was stressed that this is only the government funding and that undeferred further research occurring in industry.

Received: by YALEVM (Mailer R2.03B) id 8531; Sun, 28 Jan 90 19:04:03 EST
Date: Sun, 28 Jan 90 17:39:05 EST
From: "MOSHE GAI, (203)432 5195, FAX:(203)432 3522" <GAI@YALEVM>
Subject: Hit Probability
To: "R.L. Garwin" <rlg2@watson>

cc: K.G. Lynn, S.E. Jones
J.P. Schiffer, J. Huizenga, D. Morrison.

Dear Dick,

Your way of thinking on hit probability is so different than mine that I took a hard look on it. From day one I am used to thinking in terms of the binomial distribution, but you avoided this by knowing that the binomial distribution at, the limit of large N (N>7 or so) is, identical to the Poisson ditribution (as well as of~~f~~ course to the Gaussian distribution). ns – exact

This implies that your way of calculating hit probability should agree with mine only for large number of hits. Indeed for M>15 we agree within 0.8% on the sum of all hit probability from K=2, 3,... These are the probabilities of interest for the "cold fusion" experiment. hits vs Votes

Out of intelctual curiosity I studied the values for small multiplicities which are goverened by the low fold (K) values. Indeed for multilplicity 4 you callculate 3.77% chance of 2 or more ring counter hits and I calculate 3.12% (an agreement at the level of 20%). The agreement gets better very fast, for multiplicity of 6, you calculate a 7.69% chance of getting 2 or more hits in ring counters and I calculate 7.03% (an agreement at the level of 10%). For M=10 K>2 the agreement is at the level of 3%. mine one exact

I find these results very fascinating, and since your way of thinking is so different than most of us, I thought it might be nice to write a very short (say a brief report) paper on the two methods, the relation between the two methods and a comparison of the results from the two methods. I hope you will find it worth your while to publish this very short and simple, but most fascinating paper.

I will send you a first draft some time soon. Hope to hear your comments on it.

A few other questions:

1. May I remind you to mail to me the paper by :Menlove, Garcia and Jones for the NSF/EPRI meeting? (where it was stated that only 5% of the running time in the Los Alamos - BYU experiment was in fact relevant for the search of bursts). As you can imagine this fact is very important for my estimation of the number of bursts in our experiment, but Jones neglected to mention this fact to me.

2. Did Steve Jones sent to you (as you requested) a copy of his note to me from last Friday where he notifies me that he will not join our publication? You may expect in addition a very thick package (in single space, Font 16.7) including a very detailed criticism of the Yale-BYU-BNL experiment. You can judge for yourself, however I will state that Jones and I agreed that the only major obstacle for publication is the value of the efficiency of our array. A problem now solved by your intervention.

Some of BYU's criticism is valid, but it amounts to correction at the level of a few percent. I have yet to see an experiment without problems, that can not be improved at the level of a few percent. Other criticism arise from not understanding the experimental setup. If given a chance I can demonstrate all

the above.

3. I was happy to hear from Dave Lindley (Nature Washington office) that Laura Garwin (Nature London office) is your daughter. I had many discussions with her and she always left me impressed of her intelligence wit and sense of humor.

Best Regards Moshe Gai.

Date: 25 January 1990, 17:20:39 EST
From: (R.L.Garwin (914) 945-2555) RLG2 at YKTVMV
IBM Fellow and Science Advisor to the Director of Research
P.O. Box 218
Yorktown Hts, NY 10598
To: GAI at YALEVM
JONESSE at BYUVAX
KGL at APSEDOFF
Subject: Conclusions.
Reply-To: RLG2 at WATSON

Right now I find no significant disagreement between the efficiencies calculated by Al Anderson, myself, and Moshe Gai for 2 or more votes among the 10 ring counters, or for 4 or more votes.

Actually, from my tables, efficiency for 2 or more votes among the ring counters (with M=40 neutrons in a burst) would be 80.55% rather than the "at least 90%" cited by Gai at the foot of page 11 (Draft 2, received by me 01/23/90). Al Anderson quotes 90.45% for 50, 45.69% for 20 neutrons; my table shows 89.16% for 50 neutrons-- hardly a significant disagreement.

So if people can avoid arguing about who said what when, and who said what first, they can get down to the significance of the results. Naturally, there should be a reasonable derivation of efficiency in the paper; I offer the view that all these numbers are small enough that there is no need for power-series expansions that raise a red flag of "probabilities exceeding unity." I would think that the paper should quote some of the Monte Carlo results of Anderson in comparison with analytical results.

What does it mean? The Los Alamos results are not made more significant by including long-duration counting, if the bursts always come early in the run; so the comparison should be made with the maximum rate observed at LANL at the early parts of the run-- compare with burst RATE rather than with total bursts divided by total time. The Menlove, Garcia, Jones paper LAUR 89-3633 (10/16-18/89) states (Page 5)

> "... the majority of neutron bursts occur during warmup from LN; however the time period while the sample is between -100 C and 0 C represents less than 5% of the sample counting time."" and

> "It is highly significant that ALL of the high-yield bursts occurred during the LN warm-up at -30 C."

From Table II one can see 19 bursts with neutron numbers greater than 50-- many MUCH greater than 50, for which the ring counter should give 2 or more votes with probability exceeding 90%. Taking into account the 50% probability of 2 or more votes with neutron numbers as small as 22, one can simply say that no neutron bursts exceeding 40 occurred within the apparatus at Yale during the running time of the experiment. And one could go on to say that if the conditions were the same as at LANL, some xx events exceeding M = 40 would have been expected.

Of course, if only 2 events were anticipated, even identical source conditions would have detected the anticipated rate with an efficiency

1-e**-2

since there would be a probability e**-2 that none of the two expected events occur. This small-number probability is much more important than the inefficiency of the detector.

Of course, if the conditions can be created only at LANL, Yale doesn't have a chance... But that's what publishing experimental results is all about.

Dick Garwin

Received: from CEARN by CEARN.cern.ch (Mailer R2.05) with BSMTP id 6012; Wed, 20 Dec 89 14:37:35 GVA
Received: from dxmint.cern.ch by CEARN.cern.ch (IBM VM SMTP R1.2.2MX) with TCP; Wed, 20 Dec 89 14:37:31 GVA
Received: by dxmint.cern.ch (cernvax) (5.57/3.14)

id AA11875; Wed, 20 Dec 89 14:22:31 +0100
Message-Id: <8912201322.AA11875@dxmint.cern.ch>
Date: Wed, 20 Dec 89 14:36 GMT +1
From: MORRISON%VXPRIX.decnet.CERN@cernvax
Subject: Cold Fusion and RF.
To: rlg2@yktvmv
X-Vms-To: MINT::"rlg2@yktvmv"

Dear Roberto, 20 december 1989.

Was very interested in your account of your work and results. It was the first clear indication I had heard of RF perhaps explaining neutron bursts. As Dick Garwin had talked about such things, I sent him a message yesterday. He was so interested that within the hour he had phoned me back! He believes it is quite possible and would like to encourage you very much to continue as you have everything basically ready to enable you to do a definitive experiment.

He also had not heard of any such experiment but told me that Menlove at Los Alamos had worried about such interference effects and hence had taken precautions to shield his experiment so that he could even put a Tesla coil against the outside of his apparatus and there was no effect. Thinking about it later, this is a negative test and shows a Tesla coil at that place has no effect - but it is a limited test. What you are doing is a positive test in that you are actually looking for RF and seeing some possible indications. So Menlove's work does not necessarily rule out your conclusions - so go to it!

Also you have a gamma counter that gives no signal whereas Menlove follows the Pathological Science tradition of looking for only one effect at a time and not investigating to see if the logical consequences (gammas, tritium, helium 3 and 4, are there.

Will be in the States 1 to 21 January but will look in my Email.

Merry Christmas and a Happy New Year

Douglas.

Received: from CEARN by CEARN.cern.ch (Mailer R2.05) with BSMTP id 7128; Tue,
19 Dec 89 18:12:54 GVA
Received: from dxmint.cern.ch by CEARN.cern.ch (IBM VM SMTP R1.2.2MX) with TCP;
Tue, 19 Dec 89 18:12:50 GVA
Received: by dxmint.cern.ch (cernvax) (5.57/3.14)

id AA00434; Tue, 19 Dec 89 17:57:54 +0100
Message-Id: <8912191657.AA00434@dxmint.cern.ch>
Date: Tue, 19 Dec 89 18:11 GMT +1
From: MORRISON%VXPRIX.decnet.CERN@cernvax
Subject: Cold Fusion.
To: rlg2@yktvmv
X-Vms-To: MINT::"rlg2@yktvmv"

Dear Dick, 19 December 1989.

Yesterday Roberto Battison of Perugia came to see me about their results. They tried to repeat the Scaramuzzi type experiments and looked for gammas but found that the rate of the 2.204 MeV (Bismuth) peak to the 2.224 MeV peak was the same whether the cell was on or off.
Next they looked with a 3He counter and a gamma counter. They observed bursts of neutrons of 3 or 4 orders of magnitude above background but no gamma bursts at the same time. This was when Titanium shavings were exposed for the first time - even though the loading was very small, D/Ti = 0.004. Looking in more detail, they found the neutrons came in random spurts inside the burst. Futher when they looked at the actual charge, they found it was bipolar, i.e. each "pulse" consisted of a positive peak balanced by a negative peak so that the total area was zero.

They were at first astonished by these results and now strongly suspect that there may be RF emission from the Titanium as the deuterium goes in. They stopped in September and have not published.

I strongly urged them to start again and determine definitely if there is RF emitted. I also recommended them to adopt your suggestion of putting a microphone against the cathode and trying to pick up any cracks acoustically. It seems to me very reasonable that there could be RF emission which some equipment could pick up. Have you heard of anyone looking for it? If you could let me have any comments soon, it would be a help as if I have time (not at all sure), will try and write No.21.

Do you know if any significant changes have been made in the "draft" of the final report of the DOE Panel? - for example that peer-review be recommmended? Am a bit disappointed that the recommendations of the final report are very similar to those of the interim, but weaker, on the other hand it is much more complete and justified.

Had hoped that Norman Ramsey would follow the tradition and after his Stockholm visit, pass by CERN to give a lecture, but hear his health is not too good - great pity. We had a lecture from Dehmalt yesterday - the first half was very good.

Am going on 1st January to Houston for the DPF90 meeting which has an excellent programme, then to Berkeley for our Collaboration meeting (where I will also give my new lecture "N-Rays, Cold Fusion and Pathological Science" which was quite well received last week in London. Then to Utah where Steve Jones has invited me to BYU (he and Moshe Gai are argueing rather strongly),

This morning received a letter from Martin Fleischmann who says he received all the papers that I had sent him (though does not comment on them. He says they have completed their big paper on calorimetry and confirmed their earlier conclusions. He says he will be in Lausanne in January, but I may miss him if I am in the States.

Merry Christmas and All Best Wishes for 1990 (hope it is calmer than 1989 - though this has been a good year in many ways)

Douglas.